

MICHAEL DUMMETT

FREGE

philosophy of
mathematics



Duckworth

The philosophy of Gottlob Frege (1848-1925) is to be seen as the starting-point for the entire modern analytical movement; Russell, Wittgenstein and Quine were all profoundly influenced by Frege, and almost all analytical philosophy can be viewed as building on, or attempting to correct, his work.

In 1973 Michael Dummett published *Frege: Philosophy of Language*, the first of two volumes devoted to a comprehensive survey and discussion of Frege's philosophy, considered as roughly divisible between the philosophy of language and the philosophy of mathematics. This is the long-awaited second volume.

Until 1903, almost all Frege's work was devoted towards a single end – the construction of definitive foundations for number theory and analysis. When, in 1906, he discovered that his attempted solution to Russell's paradox would not work, he concluded that his life's work had been a total failure, the only valuable part of it being the systems of formal and philosophical logic that had underpinned it. The received evaluation of Frege endorses this assessment: it treats his philosophical logic as a fundamental starting-point for modern enquiries, but dismisses his philosophy of mathematics as a blind alley that deservedly led to contradiction. This book, expounding the arguments Frege actually used, and the conclusions he drew from them, weighs both and decides that the received view is deeply unjust. Although Frege incontestably committed a grave blunder, his philosophy of mathematics contains deep insights, and remains as necessary a starting-point as his philosophy of logic: he was the best philosopher of mathematics.

Michael Dummett is Wykeham Professor of Logic at Oxford.

For reviews of *Frege: Philosophy of Language* see the back panel of this jacket.

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MICHAEL DUMMETT

FREGE
Philosophy of Mathematics



DUCKWORTH

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for Tessa, Nathan and Nicola

Preface

A book of this title was advertised as forthcoming in Duckworth's catalogue for 1973, the year in which my *Frege: Philosophy of Language* was published. I therefore feel some need to explain why it is coming out only now to all who have been asking me, over the years, when it was going to appear. It was not in fact until 1973 that I started to write, as a separate book, this sequel to the earlier one. For the new book, I formed the plan of setting out systematically the problems of the philosophy of mathematics, and considering in order Frege's responses to them, to the extent that he said anything relevant: the architecture of the book was to be that of the subject, not of Frege's writings, that is to say of the subject as I saw it, not as Frege saw it.

I completed about two-thirds of the book in 1973. Though I was fortunate to hold, for a few years, a Senior Research Fellowship at All Souls' College, other writing commitments, including the preparation, with much help from Mark Helme and Charles Donahue, of the second edition of *Frege: Philosophy of Language* and the composition of the introduction to it, which turned into *The Interpretation of Frege's Philosophy*, prevented me from attending to the book, which remained untouched on my shelves, until 1982. In that year I was awarded an Alexander von Humboldt-Stiftung prize for study in Germany, and spent four months at the University of Münster in Westphalia, taking with me the typescript of *Frege: Philosophy of Mathematics*. There are two well-known reactions to reading what one has written long ago: to think, 'How brilliant I was then: I could never do that now'; and to wonder how one could have written such poor stuff. Mine was the second, and I started to rewrite the whole book, still on the same plan, from Chapter 3 onwards. To my disappointment, I did not finish. During four long vacations, from 1983 to 1986, I gave my main attention to trying to finish the book. Each time, it was difficult to recall just what my previous intentions had been, and each time I failed. In 1985, I decided to extract all the material on Frege's theory of real numbers and publish it as a separate monograph, including both philosophical and mathematical material, the latter including the solution, due to Dr Peter

Neumann, of the independence problem that troubled Frege;¹ I then worked simultaneously on the monograph and on the main book. But even with this excision, the latter grew beyond all reasonable size; and still I did not finish either.

During 1988–9, I enjoyed, for the first time in my life, a whole sabbatical year. I was lucky enough to spend from September to June at the Center for Advanced Study in the Behavioral Sciences at Stanford. I went hoping to complete two long unaccomplished tasks, one of them the Frege book, for which I took with me for an enormous pile of typescript and collection of discs. It was a toss-up which of the two tasks I should start on first; but I happened to select the William James lectures on *The Logical Basis of Metaphysics*. I succeeded in sending off a completed text of that book to Harvard University Press just before I left in June. I had also to revise a much shorter book, *I Tarocchi Siciliani*, in accordance with the suggestions of my then collaborator and now sorely missed friend, the late Marcello Cimino.² The result of all this was that I did not devote one minute of my time at Stanford to *Frege: Philosophy of Mathematics*, and crossed the Atlantic again with all my typescripts and discs unused.

For those who think in terms of completion rates, mine is disgraceful. ‘Completion rates’ – the very phrase is like a bell. British universities are in the course of being transformed by ideologues who misunderstand everything about academic work. The transformation is of course merely part of a transformation of society as a whole. The official stance of the ideologues is that they do not believe that there is any such thing as society; in point of fact, however, they do not believe in anything else. They are concerned, for example, with the performance of ‘the economy’: not with whether individual people are prospering, but with the economy as a distinguishable system on its own. The successful performance of the economy will grossly enrich some, and deprive others of all hope or comfort: but the aim, if one is not to take a cynical view of it, cannot be either to reward those who scramble to the top of the economic mountain or to punish those who are cast on to the scrapheap at its foot, but simply to ensure efficient functioning of the economy as such. The vision which the ideologues have of the successful functioning of the economy or of any other social mechanism is that it works well only if operated by human beings engaged in ruthlessly biting and clawing their way to the top, where they will be able to obtain a disproportionate share of limited rewards.

¹ Published in S.A. Adeleke, M.A.E. Dummett and Peter M. Neumann, ‘On a Question of Frege’s about Right-Ordered Groups’, *Bulletin of the London Mathematical Society*, vol. 19, 1987, pp. 513–21.

² I had, however, also had to devote much time to extensively revising *La Storia dei Tarocchi* which had been commissioned by Bibliopolis of Naples in 1982, and had become seriously out of date in the interim. I was compelled to give priority to this task, because the director of the publishing house, Signor Francesco del Franco, had promised to bring the book out by Christmas 1989, and wanted the revisions urgently. It has not yet appeared, but I still have hopes.

For this purpose, the people so competing with one another should not be encouraged to believe in the good of anything but themselves as individuals; if they were to believe in society as a whole, they might form ideas about protecting the weak or unfortunate that would clog the efficiency of the system. A glance at the universities as they used to be revealed a social sector not functioning in this manner; it therefore obviously could not be functioning efficiently, or justifying the money spent on it, and hence must be transformed in accordance with the model decreed by ideology.

The plan of the ideologues is to increase academic productivity by creating conditions of intense competition. Those who compose what is known, in today's unlovely jargon, as academic and academic-related staff are now to be lured by the hope of gaining, and goaded by the shame of missing, extra payments and newly invented titular status. Their output is monitored by the use of performance indicators, measuring the number of words published per year. Wittgenstein, who died in 1951 having published only one short article after the *Tractatus* of 1922, would plainly not have survived such a system. Those most savagely affected by the new regime are, as always, the ones on the bottom rung of the ladder: the graduate students working for their doctorates. The degree of Ph.D. (in Oxford, D.Phil.) fitted rather awkwardly into the system of doctorates as it had evolved in Britain out of the mediaeval one, and was originally instituted here to satisfy the needs of foreign students, for whom it was a necessary professional qualification. Only in recent years has it become an indispensable minimum qualification for British academic posts in arts subjects: candidates for them stand little chance if they cannot also show, at the start of their careers, an impressive list of publications. Relentless pressure is applied to students and their universities by the Government and its agencies – the research councils and the British Academy – to force them to complete their doctoral theses within three years of graduating; but it is hardly needed. Nervously conscious from the start that they must jostle one another for the diminished number of posts, they are anxious to jump the first hurdle of the Ph.D. degree as quickly as possible, and then rush to submit their unrevised theses for publishers to turn into books.

The universities have no option but to co-operate in organising the squalid scramble that graduate study has become, in introducing the new 'incentives' for their professors and lecturers and in supplying the data for the evaluation process. The question is to what extent they will absorb the values of their overlords and jettison those they used to have. Once more, it is the graduate students who are the most at risk, for they are in effect being taught that the rat-race operates as ferociously in the academic as in the commercial world, and that what matters is not the quality of what you write but the speed at which you write it and get it into print. It is obviously as objectionable in a capitalist as in a communist country that politicians should decide how the universities are to be run; but it is catastrophic when those politicians display

total ignorance of the need to judge academic productivity on principles quite different from those applicable to industry. Our masters show some small awareness that, as in industry, quality is relevant as well as quantity: their performance indicators are sometimes modified by the use of more sophisticated criteria, such as counting the number of references made by other writers to a given article. Frege would never have survived such a test: his writings were very seldom referred to in his lifetime. It is not, however, that quantity is not the only criterion, but that it is positively harmful. The reason is that overproduction defeats the very purpose of academic publication. It long ago became impossible to keep pace with the spate of books and of professional journals, whose number increases every year; once this happens, their production becomes an irrelevance to the working academic, save for the occasional book or article he happens to stumble on. This applies particularly to philosophy. Historians may be able to ignore much of their colleagues' work as irrelevant to their periods; but philosophers are seldom so specialised that there is anything they can afford to disregard in virtue of its subject-matter. Given their need for time to teach, to study the classics of philosophy and to think, they cannot afford to plough through the plethora of not bad, not good books and articles in the hope of hitting on the one that will truly cast light upon the problems with which they are grappling; hence, if they are sensible, they ignore them altogether.

Academics who delivered their promised manuscripts twenty years late used to cause us amusement; but it was a respectful amusement, because we knew the delay to be due, not to idleness, but to perfectionism. Perfectionism can be obsessive, like that which prevented Wittgenstein from publishing another book in his lifetime, and probably would have done so however long he had lived; but, as the phrase goes, it is a fault on the right side. Every learned book, every learned article, adds to the weight of things for others to read, and thereby reduces the chance of their reading other books or articles. Its publication is therefore not automatically justified by its having some merit: the merit must be great enough to outweigh the disservice done by its being published at all. Naturally, no individual writer can be expected to be able accurately to weigh the one against the other; but he should be conscious of the existence of such a pair of scales. We used to be trained to believe that no one should put anything into print until he no longer sees how to make it any better. That, I still believe, is the criterion we should apply; it is the only means that exists of keeping the quality of published work as high as possible, and its quantity manageably low. The ideologues who in their arrogance force their misconceived ideals upon us attempt to make us apply virtually the opposite criterion: publish the moment you can get editor or publisher to accept it. We are compelled outwardly to comply with their demands; let us inwardly continue to maintain our own values.

When I returned from Stanford in 1989, it was early June, and I still had

more than three clear months of my sabbatical to run. I plunged straight away into work on the present book. Instead of revising, compressing and tailoring the enormous amount of material I had already amassed, however, I ignored it altogether, and started writing afresh, on an entirely different plan, indeed virtually the opposite plan. Instead of arranging the book as one might arrange a systematic non-historical treatise on the philosophy of mathematics, I composed it as a close study of Frege's texts: that is, of his *Grundlagen*, followed by selected parts of the later *Grundgesetze*. Into this were to be inserted some comparative matter concerning Frege and Dedekind, and Frege and Husserl: not, however, for its own sake, but as illuminating Frege's texts. The *Grundlagen* is written with a deceptive clarity: it is in fact a very easy book to misunderstand. My original plan for my book on Frege's philosophy of mathematics had left readers without a helpful guide to the subtleties, and artfully concealed lacunae, in the argument of *Grundlagen*: I should do them much better service, I now thought, by providing one.

Furthermore, the new plan concentrated attention on what was central to Frege's philosophy of arithmetic. I had not intended, at the outset, to write a treatise of length comparable to that of *Frege: Philosophy of Language*. What had swollen the book to beyond that size was a misguided ambition to achieve comprehensiveness: I had thought I must include everything relevant to the philosophy of mathematics. A great deal of this – such as a chapter on Frege's philosophy of geometry – has now been excised. Among the casualties has been a discussion of Frege's views on the consistency of mathematical theories, in terms of his controversy with Hilbert, which had been written in 1973 and survived successive revisions intact. It had in fact been published in Matthias Schirn's collection *Studien zu Frege/Essays on Frege* of 1976: when he asked me for a contribution, I selected the most self-contained passage from the uncompleted typescript of the book. Since it has been published, and republished in my *Frege and Other Philosophers*, its omission from here is no loss. The topic is indeed of some interest; and there were other discussions, unpublished and now suppressed, on topics of similar interest. I decided, however, that the attempt to discuss everything in Frege's writings that bore on the philosophy of mathematics had resulted, and could only result, in a diffuse, rambling book. I have tried to replace it by one that goes to the heart of Frege's philosophy of arithmetic, setting aside everything not of central importance for that purpose.

Frege's reputation as a philosopher of logic, of language and of thought has grown steadily from about 1950 onwards; he is generally perceived as the founder of analytical philosophy. Not so his reputation as a philosopher of mathematics. His work in this field has tended to be equated with maintenance of the logicist thesis, and consequently dismissed as a total failure; it is ironic that, in his last years, he would have concurred with this judgement. He would have done so because he had aimed at, and for a time had believed that he

had achieved, total success; but, since no one has achieved total success, it requires explanation why that judgement should be made now. Hilbert, too, propounded a programme that proved impossible of execution as he formulated it; and his philosophy of mathematics, as a system, would have been tenable only if that programme could have been carried out: yet no one regards Hilbert's views on the subject as negligible. Probably the reason is that Frege's work does not prompt any further line of investigation in mathematical logic, unlike the modifications of Hilbert's programme studied by Georg Kreisel. It does not even appear to promise a hopeful basis for a sustainable general philosophy of mathematics: while it is appealing to be a neo-Dedekindian like Paul Benacerraf, or a neo-Hilbertian like Hartry Field, neo-Fregeanism, though espoused by Crispin Wright and by David Bostock,³ seems to most to be considerably less attractive.

Various features of Frege's work in the philosophy of mathematics have contributed to the general neglect of it. An inborn obstinacy combined with his increasing bitterness to make him ever less receptive to the ideas of others. He had a great early interest in geometry, particularly projective geometry; and in *Grundlagen* he alluded to non-Euclidean geometry in a perfectly reasonable way, categorically affirming the consistency of elliptic geometry but observing that we cannot imagine such a space. Subsequently, he became a fierce opponent of non-Euclidean geometry, descending, in a fragment of his *Nachlass* of which it is to be hoped that he was not later proud, to comparing it, as a pseudo-science, with alchemy. He allowed no merit to Hilbert's *Grundlagen der Geometrie*, nor, in his *Grundgesetze*, Volume II, to either Cantor's or Dedekind's theory of real numbers; and, although he lived until 1925, he paid scarcely any attention to the work of his successors in mathematical logic. Some explanations, psychological or intellectual, can be given for these attitudes. He continued to regard geometry as the science of physical space, and so held that there can be only one true geometrical theory. His early respect for Cantor, manifested in *Grundlagen*, was repaid by the cruelty of Cantor's mean-spirited review of that book. Yet, whatever may be said in mitigation, these evidences of the blindness and lack of generosity which were such marked features of Frege's work after 1891 combine with his great blunder in falling into the contradiction to suggest that he cannot have much to teach us.

Nevertheless, his work in this field deserves great respect. It certainly cannot be reduced to the bare statement of the logicist thesis. There is much that he found worth saying, or said for the first time, that is either obvious to us or a received part of very elementary logic or mathematics; but there is also much that remains challenging. A good deal, indeed, is patently wrong; but of which philosopher of mathematics is that not true? Despite his blindness to things

³ See C. Wright, *Frege's Conception of Numbers as Objects*, Aberdeen, 1983, and D. Bostock, *Logic and Arithmetic*, vol. I, *Natural Numbers*, Oxford, 1974, vol. II, *Rational and Irrational Numbers*, Oxford, 1979.

his contemporaries perceived, despite his unawareness of much that concerns us but wholly failed to strike him, or could not even be formulated until logic had made further advances, he is, in my judgement, the best philosopher of mathematics. This book is a historical study: but it has been written in the belief that we can still profit greatly by reflecting on what Frege wrote about the foundations of arithmetic, and therefore in the hope that it is not merely a historical study.

Oxford, July 1990

M.D.

CHAPTER 1

The Significance of Grundlagen

Die Grundlagen der Arithmetik is Frege's masterpiece: it is his most powerful and most pregnant piece of philosophical writing, composed when he was at the very height of his powers. It was written as a prolegomenon to his *magnum opus*, *Grundgesetze der Arithmetik*: a first rough sketch of Part II of that work, presented without unfamiliar symbolism and with a minimum of symbolism of any kind, in the hope of reaching as wide an audience as possible. But it occupies both a more central and a more problematic place in his work on the philosophy of arithmetic than this intention would suggest. What he did not foresee, when he was composing it, was that, in starting work on *Grundgesetze*, he would be led to make fundamental changes both in his formal logical system and in his underlying philosophy of logic. It is the system of logical and philosophical doctrines that Frege elaborated as embodying these changes which we think of as constituting his philosophy; and it was in the framework of this system that the two volumes of *Grundgesetze* were written. This suggests that *Grundlagen* should be set aside as a brilliant but immature work, and that we should study Frege's philosophy of arithmetic primarily from his *Grundgesetze*. We cannot do that, however, because he chose not to carry out, on a revised basis, a philosophical justification of his theory of natural numbers of the kind that had occupied most of *Grundlagen*: Part II of *Grundgesetze*, which corresponds to *Grundlagen* in subject-matter, is wholly formal in character, being written almost exclusively in Frege's logical notation, and thus entirely omitting the philosophical argumentation. It was not that Frege had come to consider such argumentation superfluous, for he supplies it at great length in Part III of *Grundgesetze*, which treats of the foundations of the theory of real numbers, a subject left untouched in *Grundlagen*. It must have been, rather, that he considered that readers could easily transpose the argument of *Grundlagen* into the mode of his new system of philosophical logic. If so, he gravely underestimated the difficulty of the task, which to this day creates problems not easily solved. We have no choice, however, but to treat *Grundlagen* as presenting the greater part of the philosophical underpinnings of the theory of the foundations of arithmetic expounded in *Grundgesetze*, while bearing in

mind that, if he had incorporated this material into *Grundgesetze*, he would have subjected it to substantial modification.

Grundlagen is deceptively lucid. That is not at all to say that it is deliberately misleading; only that it is so persuasively written, and so adroit in its selection of the rival views that are then so skilfully refuted, that it is easy to overlook the options that have not been presented to their best advantage, or at all, and to misconstrue the architecture of the argument as it is developed from beginning to end of the book. We have here to review the course of that argument so as to bring to light all that is not apparent on first reading.

Grundlagen is written in the framework of a Kantian terminology, not used by Frege in any of his writings after 1890, save those composed at the very end of his life. This terminology does not indicate his acceptance of any specifically Kantian doctrines: indeed, despite the tone of deep respect he frequently, though by no means invariably, adopts when speaking of Kant, he overtly discusses Kant's views almost exclusively to disagree with them. Frege's use of his terminology may be due to a special effort to make himself understood by the professional philosophers; more probably, to his simply assuming that a Kantian framework was the proper one within which to pose philosophical questions. The brilliance of *Grundlagen* makes it easy to forget that it was, after all, his first full-fledged incursion into philosophy.

The status of *Grundlagen*

The principal problem of Frege exegesis is to determine the relation between the writings of Frege's early period, up to 1886, and those of his middle period, beginning in 1891. During the years 1887–1890, he published nothing, but was engaged in thinking through afresh his system of philosophical logic and redesigning, in accordance with it, the formal system he had presented in *Begriffsschrift*. He announced his new ideas in the lecture *Function und Begriff* of 1891. The principal changes in his philosophical logic were the introduction of the far-reaching distinction between sense and reference, and the identification of truth-values as objects and as the references of sentences. The principal changes in his formal system were the introduction of value-ranges, and the obliteration of any formal distinction between sentences (henceforward called by him 'names of truth-values') and singular terms ('proper names'); the addition of a description operator was an important secondary development. During the middle period, lasting from 1891 to 1906, his thought evolved little. Doubtless much of what he wrote was newly thought out: but there is no reason to suppose that he ceased, at any later time within this period, to believe anything that he wrote for publication at any time during it. The logical basis of all the work of the middle period was presented complete and entire in *Function und Begriff*; and it scarcely altered throughout the whole period.

The early period, by contrast, was one of considerable development, during

which Frege's views changed, sometimes subtly and, in some instances, radically. To recognise this, it is sufficient to compare what Frege wrote in *Grundlagen* with the remark in the article 'Booles rechnende Logik und die Begriffsschrift', which in 1881 – only three years before the appearance of *Grundlagen* – he unsuccessfully submitted for publication, that 'individual things cannot be assumed to be given in their totality, since some of them, such as numbers for example, are first created by thinking'.¹ We therefore cannot presume that what he wrote at one time during his early period he would have continued to endorse at a later time, though it is natural to suppose that he regarded later thoughts as better. The greatest difficulty is to decide how much carried over from the early to the middle period. Naturally, when what he wrote in his middle period expressly corrected or modified something he had said in the early period, we know exactly where we are: but what when he was simply silent?

This question is particularly acute in relation to *Grundlagen*, because three salient doctrines of that book were never afterwards explicitly reaffirmed by Frege, but never explicitly denied by him, either. The first is the 'context principle', that it is only in the context of a sentence that a word has meaning. This has been much discussed: I believe that a definitive answer can be given to the greatly controverted question whether he repudiated or maintained it, and shall give that answer in its proper place. The second is the adoption by Frege, in § 3 of *Grundlagen*, of the Kantian classification of true propositions into analytic, synthetic a priori and a posteriori, and his recharacterisation of these three classes. The very object of the book is stated, in § 87, as having been to make it probable that 'the laws of arithmetic are analytic judgements and consequently a priori'; and yet, throughout his middle period, Frege never employed these or any equivalent terms. It is instructive to read the different way in which he stated the object of *Grundlagen* in the first sentence of his Introduction to *Grundgesetze*: 'in my *Grundlagen der Arithmetik* I sought to make it probable that arithmetic is a branch of logic and that no ground of proof needs to be drawn either from experience or from intuition.' Not only is this more accurate, in that to call a proposition 'analytic', in the sense of *Grundlagen*, is not to say that it is expressible in purely logical terms: more importantly, it relates, not to individual propositions, but to an entire theory, taken as a whole. It is possible that Frege came to be dissatisfied, either with the manner in which he had defined 'analytic' and 'a priori', or with those concepts themselves; if so, it is puzzling that he never said so, but, if not, equally puzzling that he refrained from ever employing them again until 1924. The third doctrine never again heard of after *Grundlagen* is that which introduced the pregnant concept of a criterion of identity: 'if we are to use the symbol *a* to designate an object', he pronounced in § 62, 'we must have a criterion which decides in all cases

¹ *Nachgelassene Schriften*, p. 38, *Posthumous Writings*, p. 34.

whether b is the same as a , even if it does not always lie within our power to apply this criterion.' This is an immensely important dictum: in this third example, it is especially mysterious that the whole topic should apparently have vanished from his thinking.

More important than whether, or to what extent, Frege continued during his middle period to maintain these three particular doctrines is the question whether or not we may take the philosophy of arithmetic expounded in *Grundlagen* to be essentially that to which he subscribed during the middle period. That the actual logical construction of the theory of the natural numbers, and of cardinal numbers generally, remained the same is beyond question, since it is repeated in *Grundgesetze* in more detail but in essentially the same way that it is sketched in *Grundlagen*: what needs to be decided is whether the philosophical ideas remained the same, allowing for the more sophisticated philosophical logic Frege had elaborated in the meantime. This question can be answered by considering the architecture of *Grundgesetze*.

The structure of *Grundgesetze*

Grundgesetze, as we have it, is divided into three Parts; but it is an uncompleted work. The division into volumes has scarcely any relation to the segmentation of the book: it looks as though Frege had an agreement with his publisher that a certain number of pages constituted a volume, and the publisher brought out a volume as soon as he had copy amounting to that number of pages. At any rate, the two volumes are of almost precisely the same length: Volume I has 254 pages of text, with 32 pages of Preface and Contents, making 286 in all, while Volume II has 253 pages of the main text, with 16 pages of Contents and 13 pages of the Appendix dealing with Russell's contradiction, which we know to have been added in proof, making 282 pages in all; perhaps Frege withdrew a section in order to make room for the Appendix. Volume I contains all of Part I and about three-quarters of Part II; Volume II contains the rest of Part II and about two-thirds of Part III: possibly Frege planned a fourth Part, or possibly Volume III, had it appeared, would have been shorter.

Volume III did not appear because Frege came to realise that his solution to Russell's contradiction, set out in the Appendix, was inadequate. The last paragraph but one of the Appendix, dated October 1902, reads as follows:

It would take us too far here to pursue further the consequences of replacing [the original axiom] (V) by [the proposed modification] (V'). It must be acknowledged that to many of the propositions auxiliary hypotheses will have to be added; but there need be no anxiety that any essential obstacles to carrying out the proofs will arise from this. It will nevertheless be necessary to check thoroughly all propositions discovered up to this point.

That of course is correct: when one of the axioms of a theory is weakened, it

becomes necessary to check that the proofs can still be carried through. The impossibility of what Frege here claimed, with misplaced confidence, to be able to do, he took nearly four years to discover. He should not be blamed for this. In 1902–3 he was occupied with his first series of articles against Hilbert's *Grundlagen der Geometrie*, in 1903–4 with his article 'Was ist eine Funktion?', and in 1905–6 with his second series of articles against Hilbert: far more serious, in 1904 his wife died. But the fact is that, as soon as Frege enquired into the question whether the proofs of the theorems of *Grundgesetze* would still go through under the weakened axiom (V'), he would have found that they did not: not even the proof of the theorem (111) that 0 does not equal 1.

From his unpublished writings, we can pinpoint the moment at which he discovered this catastrophic fact. In 1906 he began writing a reply to an article by Schoenflies on the paradoxes of set theory, which had appeared in the January issue of the *Jahresbericht der deutschen Mathematiker-Vereinigung* in that year. The unfinished draft contains a reference to an article by Korselt that appeared in the March-April issue of the same journal; as the editors of the *Nachgelassene Schriften* remark, and as his footnote references to 'this journal' indicate, Frege obviously intended to submit his reply to the *Jahresbericht*. The article was never completed and never submitted, however; but his plan for it contains an item showing clearly that, when he drew it up, he still believed in his solution to the contradiction:

Russell's contradiction cannot be eliminated in Schoenflies's way. Concepts which agree in their extension, although that extension falls under the one but not under the other.

The draft breaks off before this point. A tiny fragment is headed 'Was kann ich als Ergebnis meiner Arbeit ansehen?' ('What can I regard as the outcome of my work?'), and begins 'Almost everything hangs together with the logical notation (*Begriffsschrift*)'; it goes on to list various of his logical doctrines, remarking in passing that 'the extension of the concept, or class, is not the first thing for me'. There follows in the *Nachgelassene Schriften* a relatively lengthy 'Einleitung in die Logik' ('Introduction to Logic'), the stages in whose composition have been dated by Frege himself, the first having been written on 5 August 1906. Once during his early period, and again in 1897, he had attempted to write a systematic exposition of his philosophical logic as a whole: the *Logische Untersuchungen* of his late years are the first three chapters of a final attempt. The 'Einleitung' was his third attempt; and it follows very exactly the sketch contained in 'Was kann ich als Ergebnis meiner Arbeit ansehen?'.

It is plain enough what had happened. In the course of writing his anti-Schoenflies article, presumably as the result of a belated enquiry into the consequences for the proofs of *Grundgesetze* of the weakening of Axiom (V)

proposed in the Appendix, Frege had come to realise that his solution to Russell's contradiction did not work. As the final paragraph of that Appendix, and hence of Volume II of *Grundgesetze*, he had written:

We may regard as the fundamental problem of arithmetic the question: how do we apprehend logical objects, and in particular the numbers? What justifies us in recognising the numbers as objects? If this problem has not yet been so completely solved as I thought when I wrote this Volume, I do not doubt that the way to its solution has been found.

Now he was faced with the realisation that he had not even found the way to it. His life's work had been to construct a definitive foundation for number theory and analysis, so that their content and their justification need never again be thought problematic, and he had believed that he had succeeded: now he had to acknowledge that he had failed. His task now was to salvage from the wreck whichever of his ideas remained undamaged, those, namely, not dependent on the notion of a class or extension of a concept. This task he, with great courage, immediately undertook, even though he eventually lacked the heart to carry it through. We may thus set the date of his discovery that his solution of Russell's contradiction would not work between April and early August, 1906. We need not suppose that he ever knew that the modified system was still inconsistent, though he may possibly have suspected it: if you cannot prove that 0 and 1 are distinct, you are unlikely to be able to prove the values *true* and *false* distinct, and may even be able to prove their identity.

The late period

It is from August 1906, then, that we may date the beginning of Frege's late period. Very little was published save the three essays forming the *Logische Untersuchungen*, 'Der Gedanke' and 'Die Verneinung' in 1918 and 'Gedankengefüge' in 1923. There is very little unpublished material, even, most of it concerned with the philosophy of logic rather than of mathematics (even when it is *applied* to mathematics); Frege deliberately put aside the central problems of the philosophy of arithmetic. It was not until 1918 or 1919 that he rallied enough to address himself once more to them. He explained the matter in a letter of that period which he wrote to Karl Zsigmondy:

You will know that I have made great efforts to get clear about what we mean to refer to when we speak of 'number'. You may perhaps also know that these efforts have apparently ended in complete failure. This has acted as a continuing stimulus, which would not let the question rest within me. It went on working on me, even though, so to speak, I was no longer officially concerning myself with the matter. And this work, which has taken place within me independently of my will, has suddenly surprised me by throwing a complete light on the question.

Frege had arrived at a new philosophy of arithmetic, differing markedly from that expounded in *Grundlagen*. Arithmetic can no longer be taken as founded on logic alone; hence, as Frege maintains in two works written in the last year of his life, since it remains a priori in character, it must rest, as Kant had taught, on pure spatial or temporal intuition. In these late writings he declares, what he must for some time have believed, that set theory is an illusion generated by language, which misleads us into taking such a phrase as 'the extension of the concept *fixed star*' as standing for an object. There was not the time remaining to him to develop these new thoughts into a complete theory.

The contents of *Grundgesetze*

All this explains why no further volume of *Grundgesetze* was ever published; but one was obviously intended. If it had contained only the completion of Part III, it would have been much shorter than Volumes I and II. Part I expounds the formal system. It sets out the primitive vocabulary, formation rules, axioms, rules of inference and some definitions. It also contains an exposition of Frege's system of philosophical logic, formulated with exactitude but without argument or justification, and, in terms of that, gives in detail the semantics of the system; references to 'Über Sinn und Bedeutung',² *Function und Begriff*,³ and 'Über Begriff und Gegenstand'⁴ early in the book direct the reader to treatises in which he can find a justification of the apparatus employed. Part II contains the logical construction of the natural numbers, taken as finite cardinals, and proves various fundamental theorems concerning them and concerning the least transfinite cardinal, called by Frege '*die Anzahl Endlos*' (the number Endless), corresponding to Cantor's Aleph-0. The incomplete Part III consists of Frege's theory of real numbers, incompletely expounded. Possibly, if Volume III had ever been published, it would have contained a fourth Part, dealing with complex numbers. From the usual standpoint, it is trivial to construct the complex numbers, given the reals. Part III shows, however, that Frege wished to define the real numbers in such a way as to make the possibility of applying them to physical reality manifest in their definition, and he may have wanted to do the same for the complex numbers; when they are defined as ordered pairs of reals, with the appropriate definitions of their sums and products, their application within physics, and even the mathematical theory of functions of a complex variable, are far from immediately evident.

However this may be, there is a signal difference, already alluded to, between Parts II and III of *Grundgesetze*. Part III is divided into two halves. The

² Vol. I, Preface, p. ix fn., and Introduction, p. 7 fn.

³ Preface, p. x, Introduction, p. 5 fn. and § 21, p. 36 fn.

⁴ Introduction, pp. 3 fn., 5 fn. and 8 fn.

uncompleted second half is a formal development of Frege's construction of the real numbers; the first half is a prose justification of that construction. It is designed after the model of *Grundlagen*: alternative theories of the real numbers, including Cantor's and Dedekind's, are reviewed and criticised, so that, in the course of the critique, Frege's requirements for a correct theory emerge; at the end of the discussion, those requirements are summarised and an advance sketch is given of the construction to be developed within Frege's formal system in the second half of Part III. Part II, on the other hand, corresponds only to that second half. It consists entirely of a series of proofs and definitions within the formal system, together with the brief prose explanations that accompany the formal proofs throughout *Grundgesetze*, and completely lacks any argumentative justification for the theory such as is provided by the first half of Part III for the construction of the real numbers.

Why this asymmetry? The obvious answer is that previously suggested: that Frege was satisfied that he had already provided such a justification in *Grundlagen*, and that there was therefore no need to do it over again. No other conclusion seems possible; and it is reinforced by the repeated references to *Grundlagen* in *Grundgesetze*,⁵ together with the reference to 'Über formale Theorien der Arithmetik',⁶ a lecture given just after the publication of *Grundlagen* and fully expressing its point of view. In none of these is a note of caution sounded, like Frege's warning to the reader that *Begriffsschrift* no longer corresponds to his present standpoint.⁷ Frege of course was well aware that *Grundlagen* would need rewriting to adapt it to the later doctrines, and especially the sense/reference distinction; in 'Über Begriff und Gegenstand' he said, for example:⁸

When I wrote my *Grundlagen der Arithmetik*, I had not yet made the distinction between sense and reference and hence, under the expression 'judgeable content', grouped together what I now distinguish by the words 'thought' and 'truth-value'. I therefore no longer wholly approve of the explanation given on p. 77, although I am essentially of the same opinion.

Probably he did not realise how far-reaching such a rewriting would have to be; there is much in the book that he would have considered wrong, or at least would not have cared to say, in 1893. But we may take it as certain that he thought it sufficiently near his current views to make such rewriting redundant, and assumed that readers aware of his later doctrines would be able to

⁵ In vol. I they occur on pp. viii-xi of the Preface, pp. 1 and 3 of the Introduction, p. 14 of § 9, p. 56 of § 38, pp. 57-60 of §§ 40-6, and p. 72 of § 54.

⁶ Introduction, p. 3.

⁷ In the footnote on p. 5 of the Introduction.

⁸ P. 198.

make the necessary adjustments themselves. Hence, despite some serious uncertainties, we may consider *Grundlagen* as expressing, with fair accuracy, Frege's mature philosophy of arithmetic, not merely a superseded phase of his thinking.

CHAPTER 2

The Introduction to Grundlagen

Frege begins his Introduction by persuasively setting out the need for an enquiry of the kind undertaken in the book. Mathematicians – the mathematicians of his day – are, he says, unable to give any sensible answer to the question what the number 1 is, or what a number in general is: and so they are incapable of explaining what arithmetic is about. This is a disgrace to the science, which urgently calls for a remedy. Frege remarks that many will reckon such an enquiry not worth the trouble, supposing that the matter is already well understood; but this only shows how deep the trouble lies – we do not even know that we do not know.

Until he received Russell's letter of 16 June 1902, informing him of the inconsistency of his formal system, Frege believed that he had found the definitive solution to the problems of the foundations of number theory and analysis: the definitive answers to the questions on what our knowledge of the truths of those mathematical theories rest, and what the two theories are about. If he had really resolved these important philosophical problems, the value of his work would be beyond all doubt; since the problems remain unresolved, they can hardly be dismissed as trivial. Our task now is to answer three questions: what Frege tried to achieve; where he failed and why; and how much he actually established. Almost everyone recognises that mathematical propositions differ in status from empirical ones: they are arrived at by a process of reflection and reasoning in which observation plays no part, and they are invested with a necessity that bars us from conceiving what the world would be like if they did not hold. Frege attempted to show that *some* mathematical propositions, those of number theory and analysis which he jointly classified as 'arithmetic', had the same character as, and in fact *were*, logical propositions; he never believed this to be true of the whole of mathematics. The set-theoretic contradictions rendered his attempt a failure. They did not, however, invalidate the whole attempt from start to finish: the argument for the logical character of some simple propositions – numerical equations, for instance, or the commutative law for cardinal addition – remains unaffected. For the rest, the problem of explaining the special character of mathematical

propositions has still to be solved: the value of Frege's unsuccessful attempt lies in its pinpointing the place where the difficulty lies.

Mathematical value

The motivation for the work is set out again in §§ 1 and 2 of the main text, where Frege represents his investigation as in line with the general drive towards greater rigour in the mathematics of his time. In these sections, it is the mathematicians whom Frege is principally trying to persuade; he had chiefly aimed his remarks in the Introduction at the philosophers, although he everywhere insists that the enquiry lies on the borderline between the two subjects. There would be little point in dwelling on the reasons Frege gives for undertaking the investigation, so obvious must its interest appear to almost all with any philosophical inclination, were it not that there has been a recent movement, led by Philip Kitcher, to argue that it was indeed pointless. The argument is that, unlike the clarification of the foundations of analysis, it was not needed for the resolution of antinomies hampering the progress of mathematics. This might be thought the expression of a philistine attitude towards philosophy on the part of certain mathematicians by anyone unaware that it actually proceeded from philosophers. Such philosophers reduce themselves to the status of the repairmen of the sciences, not needed until called in to clear up some confusion that is impeding the important work of the scientists. The questions what the natural numbers are, and how we know what we assume to be true about them, are of intrinsic interest, whether or not the answers contribute to progress within number theory: since they go to determine what number theory is about, and what its epistemic basis is, they lack interest only if either number theory itself is of no value, or philosophy as a whole is devoid of interest.

There is indeed a significant contrast between the contemporary but independent work of Frege and Dedekind on the foundations of number theory; the difference could certainly be characterised by saying that Dedekind's approach was more mathematical in nature, Frege's more philosophical. Plainly, contributions to the philosophy of mathematics are not to be judged by how much they contribute to mathematics itself, any more than contributions to the philosophy of mind are to be judged by whether they advance the science of psychology. In any case, it is an illusion to suppose that Frege's foundational work was of no mathematical value, even if this judgement is restricted to number theory, with which *Grundlagen* is almost exclusively concerned, and not applied to his work on the foundations of analysis, presented only in the second volume of *Grundgesetze*. The illusion occurs for several reasons. One is the error of considering *Grundlagen* in isolation from the previous work embodied in *Begriffsschrift* – a facile mistake arising from the absence of logical symbols from *Grundlagen*; but in fact the later work depends

on the earlier, which had been carried out in preparation for it. It is not only that, in § 79 of *Grundlagen*, Frege borrows from *Begriffsschrift* the celebrated definition of the ancestral, to yield, in § 83, a definition of natural numbers as those objects for which finite mathematical induction holds good – a definition which Frege saw as serving to eliminate appeals to intuition or to specifically arithmetical modes of reasoning. It is also that the possibility of completely formalising mathematical proof underlies the entire programme, as is made clear in §§ 90 and 91: only by means of a formalisation that precludes a surreptitious appeal to intuition can we attain certainty that the theorems of number theory rest on a purely logical foundation. Plainly, inventing modern mathematical logic, and devising the very first formal system, were major contributions to mathematics under any but the narrowest circumscription of what constitutes mathematics.

A second reason for the illusion is that much of what Frege laboured to make clear is now common currency with us: no one would now regard as anything but ludicrous the explanations of the concept of number that eminent mathematical contemporaries of Frege were satisfied to give, but he criticised so trenchantly. It is possible also because the notorious failure of the most salient part of Frege's programme – the reduction of arithmetic to logic, taken as a whole – obscures the success of another part forming an essential preliminary to it. To describe him as reducing arithmetic to set theory, and then to disparage that reduction as unimportant, as is sometimes done, is to caricature both what he intended and what he accomplished. The description has, as a background assumption, what no one now would doubt, that set theory is an autonomous mathematical theory, in no way to be identified with logic. Frege valued his reduction only so long as he believed it to be a reduction to *logic*: as soon as he abandoned hope of a relatively simple means of avoiding the contradiction, he deemed it to have been a mistake to treat the theory of classes as a part of logic; the reduction thereupon ceased to interest him.

Frege's aim, as stated in *Grundlagen*, was to make it probable that the truths of number theory are analytic, in the sense he gave to that Kantian term. To do so, he did not propose to examine in turn all the theorems in some current textbook of number theory. Rather, it sufficed to provide such a demonstration for the fundamental principles of arithmetic: the rest would then follow of itself. This therefore made it necessary for Frege to identify those fundamental principles. Surprisingly, no attempt had yet been made to isolate the laws, or even the concepts, from which number theory could be developed, despite the universally admired example provided by Euclid of how this could be done for geometry. Frege's pioneering work in this regard has been overshadowed by that of Dedekind. It is a valid criticism of him that he did not actually axiomatise number theory: as is now generally known, that was done by Dedekind, whose axiomatisation was adopted by, and named after, Peano. Frege did not do this, since he had no strong reason to be interested in

distinguishing what belonged to number theory proper from its logical foundations, precisely because he believed there to be no sharp line between arithmetic and logic. Nevertheless, in *Grundlagen* and in *Grundgesetze*, he presented proofs of a number of general propositions, labelled in *Grundgesetze* 'the basic laws of cardinal number (*Anzahl*)'. Given that Frege was operating with a successor *relation* rather than with a successor *function*, his 'basic laws' had to be more explicit than the Peano axioms: he needed to prove that every natural number had a successor, and that nothing had more than one. Given this difference, his basic laws in effect comprise the five Peano axioms, not, however, isolated as an axiom-set entailing number theory as a whole: the first two, saying that 0 is a natural number and that a successor of a natural number is a natural number, and the fifth, embodying the principle of induction, are incorporated into his definition of 'natural number' ('finite number' in his terminology), from which they are immediate, rather than being formulated as theorems. The third Peano axiom, that 0 is not a successor, figures as theorem 108 of *Grundgesetze*, and as part of theorem 6 of § 78 of *Grundlagen*, while the fourth, that successor is one-many, appears as theorem 89 of *Grundgesetze*, and as half of theorem 5 of § 78 of *Grundlagen*. Frege's basic laws include some propositions concerning the number 1, which of course is not a primitive notion in the Dedekind-Peano axiomatisation (when 0 is taken as the starting-point), and others not expressible in purely number-theoretic terms, but concerned with one-one correspondence. He ought, indeed, to have seen the necessity for isolating certain of the laws as at least forming a plausibly sufficient base for the derivation of all truths of number theory; but although he did not do this, he came far closer than anyone had done previously to analysing the basis of number theory. Frege was not concerned to present number theory as an axiomatised mathematical theory in the ordinary sense, and it is therefore not surprising that, regarded from that standpoint, Dedekind's work was superior to his; it is nevertheless ludicrous to suggest that this aspect of Frege's work was of no mathematical value.

Psychologism

By a natural train of thought, Frege passes in his Introduction from explaining the motive prompting his enquiry to the first of his many polemics against psychologism. His opposition to it becomes ever harder for philosophers to comprehend, at a time when what passes in the American philosophical schools for the 'standard reading' of Frege is itself whole-heartedly psychologistic.

At this stage of his career, Frege was interested solely in the *content* of our statements, and not at all in our grasp of that content. Later, he acquired a strong interest in the latter: his notion of sense, as set out in his writings from 1891 onwards, has to do precisely with *understanding*; the sense of an expression is something that we *grasp*. What made it possible for him to go

immensely further towards a satisfactory account of understanding than anyone had done before in the history of philosophy, and certainly far further than any of his contemporaries, was, however, that he had *started* with the notion of content, and that he therefore fashioned a theory of sense in accord with that, rather than trying to explain content in terms of our grasp of it.

At the time Frege was writing, psychologism was not a mere tendency, but an explicit philosophical doctrine, consciously held and widely subscribed to: not Frege's attacks on it, but those of Husserl in his *Prolegomena zur reinen Logik* of 1900, the first part of his *Logische Untersuchungen*, first loosened its grip on the German philosophical community. Even as no more than an unconscious inclination, however, it is rather a natural one for a professional philosopher. When a child asks an adult what 'sister-in-law' means, the adult will not refer to any inner mental processes that accompany hearing the word 'sister-in-law', but will tell the child in what cases one person is rightly said to be another's sister-in-law. That was what the child needed to be told: and the adult tells him that because he is not concentrating on the notion of meaning, but takes it for granted. The philosopher, on the other hand, is, very properly, perplexed by the notion of meaning. He quite rightly regards it as an extraordinary thing, demanding explanation, that words – noises that issue from our mouths or marks we make on paper – should *have* meanings. He naturally thinks that their possessing them depends on what goes on in our minds. All that *physically* occurs when two people converse is that they alternately make certain noises: the fact that they are exchanging thoughts, asking questions, giving information, raising objections, etc., must have to do, the philosopher concludes, with what takes place in their minds, where a connection has been established between the noises that they emit and the ideas they thereby express and convey. What makes the difference, he thinks, is that each *interprets* the utterances of the other; and so he is driven to concentrate upon the inner process of interpretation. The philosopher's disposition to think in this way is reinforced by his addressing himself to words whose meanings are more fundamental, and frequently more abstract, than 'sister-in-law', and therefore harder to explain; either in conformity to a general methodology, or without explicitly noticing what he is doing, he substitutes for an explanation of content an account of mental operations accompanying the use or hearing of the word or leading up to a grasp of its meaning.

Frege made the simple observation that anyone would make if offered a psychologistic account when he asked what 'sister-in-law' meant: you can make no *use* of the purported 'explanation'. You cannot use it to decide that someone is, or is not, the sister-in-law of somebody else; and so it has not captured the content of the word. If a mathematical term is explained psychologically, you cannot appeal to the explanation to prove a theorem involving it: definitions, to serve their purpose, must be fruitful in at least this sense, that we can use them to determine the truth or falsity of a statement

containing the expression defined. Once we recognise that, we see that psychology – the description of inner mental operations or of their hypothesised physiological correlates – has no place in mathematics or logic.

Frege, concentrating on the *content* of the expressions he was concerned to analyse, determined that his definitions should be fruitful in the manner that definitions in mathematics – at least, those given after the first two pages – ordinarily were: and this meant that they must serve to determine when the sentences containing them were true and when they were false. For that, on Frege's view, was what distinguishes *thoughts* from everything else, namely that they may meaningfully be called 'true' or 'false'. Everything else is irrelevant to the content of a thought – to what thought it is: when – and only when – it is determined under what conditions a thought is true, it is thereby determined what its content is.

Grundlagen is, of course, a work of Frege's early period, when he operated with an undifferentiated, and not very precisely analysed, notion of content. In the interval, from 1886 to 1890, between his early and middle periods, he developed his famous theory of sense and reference, which he expounded in the middle period (1891–1906). Because he had first concentrated on the notion of content, his theory of sense was elaborated from that model. Thoughts – the senses of sentences – are, on his account, intimately connected with the notion of truth – a notion belonging to the theory of reference. Our grasp of the sense of an expression is our way of apprehending what its reference is – a particular way, out of various possible ways; and our grasp of the thought expressed by a sentence is constituted by our apprehension of the condition for it to be true.

The notion of understanding – of a grasp of sense – is of crucial importance to a philosophy of either thought or language. Thoughts can be conveyed, and are conveyed by language; a philosophical account of communication is obviously impossible without an account of what understanding is. That is why Frege was quite right to interest himself in the notion in his middle and late periods. Now understanding is a grasp of content: sense can therefore be explained only as a way in which content is grasped. We can arrive at a plausible account of sense only if we first have a workable conception of content – of that which is grasped; and that is why Frege arrived, for the first time in the history of philosophical enquiry, at what was at least the beginnings of a plausible account of sense, and thus of understanding. Those who *started* with the conception of the inner grasp of meaning floundered in confused descriptions of irrelevant mental processes, achieving nothing towards explaining either the general notion of meaning or the meanings of specific expressions.

The notion of content, as used in the foregoing discussion, is ambiguous: it wavers between the realms of reference and of sense. That was unavoidable: it was indeed ambiguous as Frege used it in his early period, and it was his

perception of that ambiguity that drove him to make the sense/reference distinction. The notion of content cannot be definitely located in either realm: the content of a sentence is obviously not identifiable with its reference, which is merely its truth-value, nor with any structure that might be imagined as made up out of the references of its parts, somehow held apart from one another so that function and argument refrain from yielding the corresponding value. Nor can the notion of content be straightforwardly identified with the later notion of sense, because it is not conceived as correlative to an act of *grasping* it. That is why Frege always later said that he made the distinction between sense and reference *within* the notion of content. When he used the latter notion in his early period, however, his attention was almost always directed outwards, as it were, on what was needed for the truth of a statement, rather than on our apprehension of that condition in accordance with the manner in which it was stated.

That is not to concede that the notion of understanding can rest on quite so objectivist a base as Frege believed. Sense, on Frege's account, is our way of grasping what the reference is; and the reference is something in the objective world, quite independent of us or our awareness of it. To have a Fregean grasp of sense, we must have a conception of what it is for a statement to be true, independently of our means of recognising its truth. There appears, however, to be no non-circular way of explaining what it is to have such a conception, or hence of giving an account of understanding that does not presuppose what it purports to explain. If there is not, a possible remedy is to replace the notion of knowing what it is for a statement to be true by that of knowing what would rightly lead us to recognise it as true. Such a substitution of what may broadly be called a verificationist theory of meaning for Frege's truth-conditional one would greatly narrow the gap between sense and reference. It would nevertheless preserve the essential structure of the Fregean theory, since it would still explain meaning as a communally recognised feature of expressions, and understanding as the grasp of that feature, rather than characterising meaning in terms of mental operations taken as constituting understanding.

There are two lacunas in Frege's account, at opposite ends: one at the end of sense, and the other at that of a speaker's grasp of sense. What constitutes a word's having, or expressing, the sense that it does, that is, its sense in the language to which it belongs? And in what does an individual's grasp of that sense consist – either his apprehension of the sense in itself, or his attaching that sense to that word? Frege himself did not so much as mention the former of these two questions; he mentioned the latter only to brand it a mystery and relegate it to psychology. His theory presents sense as something to be grasped, a grasp of sense being either a piece of knowledge or something closely analogous to one. This circumscribes what the sense of an expression can be: it must be something that *could* be the content of knowledge or of apprehension,

and it must be plausible to attribute a grasp of it to the speakers of the language in virtue of their understanding the expression. Frege respected these constraints: it is in fact they which force the distinction between sense and reference. He confined himself, however, to giving an account of *what* we grasp, leaving it for psychology to explain the manner of our grasping it.

The fundamental principle of analytical philosophy is the priority, in the order of explanation, of language over thought: the only route to a philosophical account of thought is through an analysis of its expression in words or symbols, that is, a theory of linguistic meaning. So long as this principle remained in place, it was possible to fill the two lacunas in *different* ways, or, more precisely, to make the means of filling the second depend on that of filling the first. On such an account, to be found in its clearest and most explicit form in Wittgenstein, the sense of an expression consists in its role within the complex social practice constituting the communal use of the language, a practice open to view and not in itself involving any hidden mental operations. An individual speaker's grasp of that sense then becomes one ingredient in his ability, acquired by training, to engage in that practice. On this approach, if any explanation were needed of a possession of this ability, it would not belong to the philosophical order, but would properly pertain to psychology; such an explanation would be altogether irrelevant to a philosophical account of linguistic understanding, and hence of thought. In this way, the structure of the Fregean theory is fully safeguarded. A reversal in the order of dependence between the ways of filling the two lacunas does not necessitate abandoning the fundamental principle of analytical philosophy; but it is one step in a retreat back to psychologism. This reversal involves taking, as the basic notion, not that of the language common to a community, but the idiolect of a single individual. A speaker's mastery of his own idiolect is taken as consisting in, or at least resembling, a knowledge of a theory of meaning for it, and his grasp of the sense of a particular expression as a constituent of that complex knowledge. The sense of the expression in the common language can then be explained as its sense in a majority of a range of overlapping idiolects. The irreversible retreat to psychologism takes place when, as increasingly within the analytical tradition, the fundamental principle is jettisoned, and thought treated as prior, in the order of explanation, to language. This development is due, in part, to the instability of the intermediate position: since individual speakers manifestly have no explicit knowledge of a theory of meaning for their idiolects, the questions whether they can in any sense be said to know such a theory, and, if so, what constitutes their knowledge, or, if not, in what simulation of knowledge their linguistic competence consists, become pressing. Since all attention is focussed on the abilities of the individual subject, the temptation becomes irresistible to attempt a *direct* explanation of that subject's processes of thought, considered as unmediated by their linguistic expression, and append to it a hypothesis concerning the connection he then makes

between the words of his idiolect and features of his thought. At this stage psychologism has in effect been fully reinstated, even if, as with many nineteenth-century thinkers, scientific respectability is thought to be maintained by ritual obeisance to materialism and an assurance that, ultimately, all will reduce to neurophysiology.

It is uncontroversial that much of philosophy is concerned with the analysis of concepts; and certainly Frege's *Grundlagen* is occupied to a large extent with the analysis of numerical and arithmetical concepts. If a sound analysis is to be given of a concept or set of concepts, it must proceed in accordance with a correct conception, even if only implicit, of what the analysis of a concept requires. Any such conception stands to be vindicated by the general philosophy of thought; when the philosophy of thought is approached via the philosophy of language, its vindication will be provided by the theory of meaning. It is for this reason that the theory of meaning acquired so fundamental a place in the architecture of philosophy as practised by the analytical school: a correct theory of meaning will determine what is to count as an adequate analysis of the meaning of an expression, and hence of a concept. The view that the meaning of an expression in a language consists in its having identical or similar meanings in a large number of idiolects, and that its meaning in an idiolect is to be characterised in terms of the workings of the individual subject's mind, entails that the analysis of a concept must ultimately be given in psychological terms: precisely the view combatted by Frege in the Introduction to *Grundlagen*. Nevertheless, adherents of the new psychologism are bound to concede that, although Frege's remarks are couched in highly general terms, he was right at the level with which he was directly concerned. His principal object in *Grundlagen* was to determine the *justification* of the propositions of number theory, and of others involving the natural numbers. In the Introduction, he argued that, for this purpose, psychological accounts are valueless, and must be replaced by definitions that specify the contribution made by the expression defined to the condition for the truth of a statement in which it occurs; considerations about the mechanism of an individual subject's grasp of its meaning are beside the point. His arguments are so compelling that modern psychologicistic meaning-theorists cannot refuse to allow room for analysis at the level he was urging as the only relevant one. Any adequate meaning-theory must, after all, acknowledge the place of the concept of truth, and recognise that, for a great many statements belonging to the common language, and above all for those of mathematics, the criteria for their truth are held in common. Frege did not deny the possibility, or even the value, of psychological investigations. 'It may indeed be of some use', he says on p. vi of his Introduction, 'to examine the ideas and changes of ideas that occur during mathematical thinking'; but he adds, 'psychology should not imagine that it can contribute anything to the foundation of arithmetic'. No reassess-

ment of the attack made by Frege and by Husserl upon psychologism can afford to overlook the incontestable truth of that dictum.

The conflict between Frege and Husserl over psychologism

In Chapter 1 of his recent book on Husserl,¹ David Bell puts up a valiant defence of Husserl's *Philosophie der Arithmetik* of 1891, rating it as giving a better account of its subject than Frege's *Grundlagen*, published seven years earlier and criticised by Husserl in his book. Bell's motivation for this lies in his repudiation of the conventional view that Husserl's first book was imbued with the psychologism of which Frege, in his review of the book in 1894, perceived it as a salient example. As we have seen, Husserl later rejected and attacked psychologism in his *Prolegomena zur reinen Logik* of 1900; a prevalent opinion is that he was prompted to his change of view by the severe criticisms of Frege in his review. Bell thinks, on the contrary, that there was no change of view on this matter. According to him (p. 81), the psychologistic component of the *Philosophie der Arithmetik* is far more restrained than Frege misunderstood it as being, and Husserl's position in the *Logische Untersuchungen* is indistinguishable from that of the earlier book. It is a pity that Bell devotes only a hurried paragraph to the *Prolegomena*; he surely owed his readers an explanation of what he thought Husserl was attacking, if not the doctrine that he had formerly held. Husserl's footnote, in which he retracts his criticisms, in his first book, of Frege's anti-psychologism, tells in favour of the more usual interpretation;² but Bell seems to have overlooked it, wrongly saying (p. 137) that Frege is mentioned only once in the whole of the *Logische Untersuchungen*. An author who began in a condition of deep philosophical confusion, but then, by heroic efforts, eradicated that confusion, is certainly more interesting than one who, throughout his life, remained in a state of confusion. That is why it is important for Bell to demonstrate that there was no confusion in the *Philosophie der Arithmetik*. He does not succeed.

Bell's general defence of Husserl is that he was concerned first to give a 'theory of our *concepts and intuitions of numbers*' (p. 61), and distinguished this from an account of what the numbers are, which he intended to go on to explain in a second volume never published or composed. This is not well stated: for an accurate account of the concept of number would tell us all we had a right or need to ask about what numbers, in general, are. Bell means, I think, that the first (and, as events proved, only) volume of the *Philosophie der Arithmetik* was devoted to the task of explaining how we form our concepts of individual cardinal numbers and of number in general; the second volume would then have gone on to explain what the numbers are. Certainly, this description of the content of the first volume agrees very well with what is to

¹ David Bell, *Husserl*, London, 1990.

² Footnote to § 45 of the *Prolegomena*, which formed part I of the *Logische Untersuchungen*.

be found there; but, when the project as a whole is so explained, its absurdity is manifest. For to explain what the numbers are is just to characterise the general concept of number, so that the project would be first to say how we form that concept, and then to say what the concept is. This is evidently impossible, however: there is no way of giving an account of how we form a concept in advance of attaining clarity about what that concept is.

In fact, Bell's understanding of Husserl's project receives no support from Husserl's text. In his Preface, Husserl promises to devote Part 1 of his second volume, not to some ontological counterpart to the psychological investigation of Volume I, but to what he calls 'quasi-numbers', i.e. 'negative, imaginary, rational and irrational numbers', and Part 2 to the question whether it is the natural numbers or one of these other number-domains that is governed by 'general arithmetic in its first and original sense'.³ There simply is no such distinction in Husserl's book between the number-concept and the objective number as Bell strives to make us believe; Frege was right in his review to say that Husserl obliterates 'the boundary between the subjective and the objective',⁴ so that no clear differentiation between a number and a number-concept remains possible. It is clear that Husserl took a pure number (*reine Anzahl*) to be an aggregate of featureless units, obtained from a more determinate aggregate by mentally abstracting from the particular features of its members:⁵ the very conception whose incoherence was, as we shall see, demonstrated so conclusively by Frege in *Grundlagen*, §§ 34–44, and the terminus of the Husserlian process of forming the concept of a particular number. When Husserl says that 'the arithmetician does not operate with the number-concepts as such, but with the . . . objects of these concepts', it is not to introduce some objective entities distinct from his psychologically obtained number-concepts, but to suggest that the mathematician uses '5' as a variable ranging over five-membered sets.⁶ Bell's defence is based upon a distinction Husserl never draws.

If Husserl had proposed an account of the process of forming the concept of number as something that could stand on its own, before a subsequent account was given of the concept itself, the mistake would have been bad enough: in fact, he *substitutes* his account of the process of concept-formation for a delineation of the concept. It is above all in making this substitution that psychologism is objectionable; and it is precisely for this reason that Frege opposes it so vehemently. The characteristic expression of his anti-psychol-

³ E. Husserl, *Philosophie der Arithmetik*, Halle, 1891, pp. vii–viii. I give page references to this original edition for the sake of any whose libraries may contain it, but not the more accessible reprint in *Husserliana*, vol. XII, ed. Lothar Eley, the Hague, 1970, which, on pp. 565–9, supplies a table of correspondences between its pagination and that of the original.

⁴ Review of Husserl, p. 317.

⁵ See e.g. the essay 'Zur Lehre vom Inbegriff' of 1891, reprinted in *Husserliana*, vol. XII, ed. L. Eley, 1970, pp. 385–407, particularly p. 389.

⁶ *Philosophie der Arithmetik*, pp. 201–2.

ogism in the *Grundlagen* is the warning in the Introduction (p. vi) not to 'take a description of the way in which an idea arises for a definition'. Definitions must be certified as genuine by being fruitful, which means that we may appeal to them in the course of proving theorems (p. ix); but 'a description of how we arrive at the object or concept in question' can never serve this purpose (p. viii), and so cannot be substituted for genuine conceptual analysis. In particular, therefore, 'a description of the inner processes that precede the formation of a judgement of number . . . can never replace a genuine determination of the concept' (§ 26). Bell might object that Frege *does* concern himself with concept-formation in the *Grundlagen*, above all in the celebrated claim that by construing '*a* is parallel to *b*' as an identity-statement, 'we carve up the content in a way different from the original one, and thereby attain' the 'new concept' of a direction (§ 64). But what differentiated such an account from one of the type used by Husserl was, as we shall see, that it did not serve in place of a true definition, but as a guide to arriving at one.

In the philosophy of Frege's middle period (1891–1906), a more rigid doctrine marks the boundary between psychology and logic. The notion of sense is correlative to that of understanding, that is, of grasping a sense: what may be attributed to sense is constrained by the principle that sense can be grasped. But, in logic, we are concerned only with what the sense *is*; the mental act of grasping it, hard as that may be to explain, is a matter for psychology, and is of no concern to logic. Now understanding either is a species of knowledge or is akin to knowledge: so, although Frege never spoke of it as knowledge, we may express the point untendentiously by saying that the concern of logic, or, as we should say, of the theory of meaning, is solely with *what* a speaker knows about an expression in virtue of knowing the language, with the *content* of his knowledge, and not with the manner in which he knows it, or in what his knowing it consists. This view of Frege's, as it stands, is surely not quite right: but we shall not properly appreciate why he came to think it if we do not view, as they truly were, the psychologistic doctrines prevalent in his time, such as those advanced in Husserl's *Philosophie der Arithmetik*.

Methodological principles

Frege concludes the Introduction to *Grundlagen* by enunciating three methodological principles: the psychological is always to be sharply separated from the logical, the subjective from the objective; the meanings of words must be asked after only in the context of sentences, not in isolation; and the distinction between a concept and an object is always to be kept in view. The second of these is the celebrated context principle, to be discussed at greater length at the point at which Frege applies it. He was vividly conscious of its connection with his repudiation of psychologism. To ask after the meaning of a word in

the context of a sentence in which it may occur *is* to explain it in terms of its contribution to what is required to determine such a sentence as true. To ask after it in isolation is, as he remarks, at least to court a severe temptation to explain it in terms of the mental images it evokes or the mental acts that accompany our contemplation, or subserve our grasp, of it.

The third principle is unconnected, but embodies Frege's rejection of the procedure of postulation in mathematics. Definition of a general term, such as 'porcupine' or 'unicorn', cannot of itself guarantee the existence of an object to which it applies. That, if it is possible at all, requires independent demonstration; and this applies as much when the definition rules out there being more than one object to which the term applies as in the general case. This is perfectly obvious when the general term is an empirical one, and equally *within* a mathematical theory. When the point is made concerning the foundations of such a theory, it raises the whole question on what basis we recognise the existence of mathematical objects, a problem with which Frege wrestled, and by which he was in the end defeated. That cannot alter the need for distinguishing between the specification of a general concept, whether a mathematical one or not, and the assertion that there is an object falling under it. The third of Frege's principles offers the least opportunity for controverting it.

CHAPTER 3

Analyticity

In § 3 of *Grundlagen*, Frege gives his own characterisations of the two Kantian dichotomies, the a priori versus the a posteriori, and the analytic versus the synthetic. He claims, in a footnote, that he is not wishing 'to assign a new sense' to the terms, but 'only to hit off what earlier writers, and Kant in particular, have intended'. This somewhat disingenuous disclaimer is corrected in § 88, where Frege says that Kant was guilty of 'too narrow a definition of the concept' of analytic judgements, and that 'on the basis of his definition, the division into analytic and synthetic judgements is not exhaustive', although he concedes that 'he seems to have had some inkling of the wider concept' employed in *Grundlagen*. Frege wavers, in § 3, between treating the Kantian terms as applying to 'judgements', 'propositions' (*Sätze*) and 'truths'; he is explicit that none of them applies to a false proposition. The basis of his classification is the justification for the judgement: not how we in fact know the proposition to be true, but the best justification of it that *could* be given. He regards such a justification as a proof: and he envisages the proof as deductive in character, with the crucial exception that, in the course of it, appeal may be made to definitions of the terms involved. When such an appeal is made, we must also take account of 'the propositions on which the admissibility of a definition depends'; the proofs of any such auxiliary propositions must be included in the proof of the proposition into whose status we are enquiring.

Austin's example has here been followed of rendering Frege's word '*Satz*', as used in this section of *Grundlagen*, by the ambiguous term 'proposition', whose ambiguity it indeed shares in German. It is plain, however, that, in allowing explicitly for definitions to be invoked in the course of the deduction, Frege shows that he is characterising the status of sentences, not of their contents. A definition states what an expression is to mean, or else what it is already used to mean: a concept is not open to any stipulation. Concepts may be analysed, but not defined; it is words and symbols that are the subjects of definitions, and what is derived by means of them must be a verbal or symbolic sentence.

Given such a proof, the status of the proposition will depend upon the initial premisses of the proof. If the justification is complete, the initial premisses will not themselves be capable of proof. In somewhat imprecise language, Frege distinguishes among them between what he calls 'facts', and explains as 'unprovable truths devoid of generality, the contents of which are predications about particular objects', and 'general laws' which, he says in a phrase almost identical with one used by Lotze, 'themselves are neither capable of proof nor need one'.¹ If the initial premisses of any justification of the proposition include particular facts, then the proposition is a posteriori; if it can be proved from general laws alone, it is a priori. Among a priori propositions, analytic ones are distinguished by being derivable from general logical laws, together, of course, with the definitions to which appeal may always be made in the course of a justification. If, however, the initial premisses, though consisting exclusively of general laws, necessarily include some 'which are not of a general logical nature, but relate to some special domain of knowledge', the proposition, though a priori, is synthetic. The implicit characterisation of a logical proposition is thus that it involves only terms of universal application, whose use in no way delimits the domain in which the proposition holds good; they are, in a later terminology, 'topic-neutral'.

With uncharacteristic carelessness, Frege has framed his definition so as not to cover the initial premisses themselves. The criticism cannot be evaded by declaring a one-line derivation whose premiss coincides with its conclusion a limiting case of a proof, since Frege says explicitly that neither the particular facts nor the general laws are provable. An obvious extension of his definition would rate the particular facts as a posteriori, the general logical laws as analytic and the general laws belonging to a restricted domain as synthetic a priori. A more serious failure on Frege's part to make his own classification exhaustive is discernible if it is a classification of true propositions rather than of judgements, for he makes no allowance for there being true propositions that cannot be known at all. In the Preface to *Grundgesetze* he insisted that the truth of a proposition is independent of its being recognised to be true: 'being true is something different from being held to be true, whether by one, by many, or by all, and can in no way be reduced to it.'² It follows that the meaning of the proposition must be given in terms of what will render it true, conceived of as independent of how we recognise it as true; it therefore requires special argument if it is nevertheless to be maintained that every true proposition is capable of being known by us to be true. Frege offers no such argument: he therefore has no ground to rule out the possibility that there are truths that cannot be known either a priori or a posteriori.

A whole epistemology is implicit in Frege's refashioning of the Kantian

¹ In his *Metaphysik*, Leipzig, 1879, § 1, Hermann Lotze speaks of 'truths that neither need nor are capable of proof'.

² Vol. I, p. xv.

trichotomy of judgements; perhaps his later abstention from the use of the terms 'analytic', 'a priori' and their contraries is due to dissatisfaction with it. As he remarks in the footnote to § 3, 'from mere individual facts nothing follows': if our knowledge is not to be confined to such individual facts as we observe to hold, it must include some general truths. The main text assumes that all justification proceeds by deductive reasoning. Any judgement that can be justified at all can be justified by a deductive derivation: he does not allow for the possibility of any other form of justification. The footnote indeed allows that empirical induction may establish the truth of a physical law only with probability; it is left unclear whether this would constitute a justification of the law itself, or only of the proposition assigning it a certain probability. In the unpublished fragment 'Logik', perhaps written about when *Grundlagen* was published, he indeed admitted the necessity for non-deductive justifications:

Now the grounds which justify the recognition of a truth often lie in other truths already recognised. If truths are to be recognised by us at all, however, this cannot be the only kind of justification.

He then qualified this concession by adding:

There must be judgements whose justification rests on something different, if indeed they need a justification at all.³

Now Frege unwaveringly believed that any deductive proof must have a starting-point in the form of initial premisses. A complete justification must therefore derive from premisses of which no further justification is possible: propositions that we know without the need, and without the possibility, of proof. If we can claim to know anything more than particular facts, therefore, if we know any general truths, we must know, without the need or possibility of proof, some fundamental general laws. In the footnote, he cites the principle underlying empirical induction as an instance of such a general law that is not logical in nature; we know that he held the axioms of Euclidean geometry to have a similar status. Frege believed all this because he consistently rejected the legitimacy of deriving a consequence from a mere supposition: all inference must be from true premisses. This excludes the use of reasoning under a hypothesis subsequently to be discharged by a rule of inference such as *reductio ad absurdum*. In ordinary practice, we apply this rule by first stating a hypothesis, such as 'Suppose 2 has a rational square root'. We then reason under this hypothesis, drawing consequences dependent on it; when we finally derive a contradictory consequence, such as that some integer is both odd and even, we conclude to the falsity of the hypothesis, our conclusion of course no longer being governed by it. According to Frege, however, this is not a correct account

³ *Nachgelassene Schriften*, p. 3, *Posthumous Writings*, p. 3.

of any legitimate inferential procedure. On his view, any step in our reasoning has to be asserted outright: what figured in the foregoing description as the initial enunciation of a hypothesis should be considered as the formulation of the antecedent of each of a series of conditionals forming every step in the argument except the final one. The penultimate step will then be of such a form as 'If 2 has a rational square root, some integer is both odd and even', from which we then derive our conclusion '2 has no rational square root'.

Hilbert and Russell both followed Frege in formalising logic in accordance with this principle. Such a formalisation, exemplified both in *Begriffsschrift* and in *Grundgesetze*, does not directly address itself to the analysis of deductive inferences, but constitutes a formal theory of logical truth: it begins with the axiomatic stipulation of certain logical truths, and derives others by means of a restricted number of rules of inference. For sentential or first-order logic, the logical truths are represented by valid formulas, in higher-order logics, by sentences formulated in purely logical terms: in either case, the specification of what is to count as a valid argument from non-logical premisses to a non-logical conclusion is only supplementary to (though not uniformly derivable from) the central theory, which is a theory of logical truth.

All this was changed by Gerhard Gentzen, who did not share Frege's quite unjustifiable hostility to rules of inference that discharge hypotheses, and to the reasoning under hypothesis that leads up to an application of such a rule. The result was his formalisation of logic in natural deduction systems, whose direct concern was with rules of inference and which dispensed with axioms altogether. In the light of such a formalisation, logically true sentences are a mere by-product of the procedure necessary for drawing non-logical consequences from non-logical premisses: they arise simply by successively discharging all hypotheses. It is thus not true that every deductive argument requires initial premisses. Framed in terms of a natural deduction formalisation of logic, analytic propositions could be defined as those logically derivable, with the help of definitions, from the null set of premisses; such a formulation greatly reduces the analogy between them and synthetic a priori propositions, as Frege conceived of them.

A finer classification

Frege classifies true propositions according as they *can* be known a priori or can be known only a posteriori, omitting the possibility that they cannot be known at all. He emphasises that their status does not depend on the grounds on which they are in fact accepted:

When we call a proposition a posteriori or analytic in my sense, we are not making a judgement about the psychological, physiological or physical circumstances that have made it possible to form the content of the proposition in our consciousness,

nor about the way, perhaps erroneous, in which someone else has come to take it to be true, but about the ultimate ground on which the justification for taking it to be true depends.

Some doubt is cast by the qualification 'perhaps erroneous'; but it is natural to take Frege as meaning that an a priori proposition may be known a posteriori: otherwise the status of the proposition would be determined by any *correct* justification that could be given for it. This suggests that a priori propositions can be further subdivided into those that can be known a posteriori, and those which, if known at all, can only be known a priori. To avoid triviality, we must here exclude derivative knowledge – knowledge depending upon that of another or on the subject's memory of having had that knowledge in the past. If I know the truth of a theorem because I have been assured of it by a trustworthy mathematician, my knowledge is not a priori: since *any* truth may be known by testimony, and hence a posteriori, we may disregard such knowledge in the present context.

Even on this understanding, the existence of propositions of the former kind cannot be questioned. If I know that John Trevor was born at Leighton Buzzard, I shall agree with anyone who says that he was born at Leighton Buzzard if anyone was; and I shall then also agree with anyone who says that there is someone who was born at Leighton Buzzard if anyone was, but very likely without noticing that (by the standards of classical logic) this is analytic in Frege's sense: *my* reason for assenting to the proposition is that I know someone who actually was born there. A sentence which instantiates a valid formula of first-order, or even of sentential, logic may be recognised as true in the same way as a similar sentence that is not logically true, by evaluating it in accordance with its structure after determining the truth-values of subsentences or the applications of constituent predicates; one need not notice, in the process, that the outcome would have been the same whatever the subsentences or the predicates. Conversely, certain truths, such as 'There are seven days in the week' and 'April comes after March', are constitutive of the meanings of the words used to express them, and hence are not only true a priori, but could only be known a priori.

Into which subclass should we put numerical equations? If I use my pocket calculator to add 56179 and 43286, it appears that I now have a posteriori knowledge of an a priori truth. This case differs, however, from that of an instance of a valid formula involving empirical predicates. If a sentence of the latter kind is recognised as true by determining the application of the predicates and the truth-values of the subsentences, the recognition of its truth has been effected in accordance with the way in which its meaning was given. It is, as it were, an accident, not intrinsic to our grasp of its meaning, that it was wired up in such a way that it would have come out true whatever the extensions of its predicates and the truth-values of its subsentences; that is why we can

understand the sentence and recognise its truth without noticing that it is analytic. If, on the other hand, we take the meaning of a numerical equation such as ' $13! = 6227020800$ ' as given by the rule for computing the function, it is an accident that it should be possible to make an electronic machine mimic the computation procedure. The interesting principle of classification is not whether we can know the truth of the proposition a posteriori, but whether we know a priori that, if it can be known at all, it can be known a priori: we know this of the numerical equation, but not of the instance of the valid formula. We might say of propositions of which we know this that they are 'claimants to apriority'. A claimant to apriority need not be known to be true, or even be true; but if we know it a posteriori, we also know a posteriori that it is true a priori.

Epistemic and ontic modalities

Frege classified truths according to an epistemic principle, that is, by reference to how we can know them. 'A priori' and 'a posteriori' are naturally taken, as Kant took them, as epithets which, in the first instance, qualify our knowledge; but Frege understood 'analytic' and 'synthetic' in an equally epistemic sense. In this, too, he was essentially in agreement with Kant, since although, in the *Kritik der reinen Vernunft*, Kant defined an analytic judgement in terms of the relation between the concepts expressed by its subject and predicate, and a synthetic judgement as one that was not analytic, his immediate comment was that only synthetic judgements extend our knowledge.

Bolzano, in his *Wissenschaftslehre* of 1837, had taken Kant to task for defining any of these concepts by reference to knowledge. The details of his classification of propositions is of less significance than the principle on which it is based. In § 133, he distinguished 'conceptual' from what he variously called 'perceptual', 'empirical' or 'intuitive' propositions. He accepted Kant's distinction, among ideas, between concepts and intuitions, modifying it only by the admission of mixed ideas, compounded of both. For him, a conceptual proposition was one involving only pure concepts, an intuitive proposition one involving some intuition. He went on to remark that this distinction happens 'nearly to coincide' with that drawn by Kant between a priori and a posteriori judgements, 'since the truth of most conceptual propositions can be decided by pure thought, while propositions that contain an intuition can be judged only by experience'. He nevertheless objected to Kant's having replaced the former distinction by the latter: 'the former rests, not on the relation of propositions to our cognitive faculty, but on their intrinsic characteristics.' In support of this claim, he observed that, by stating that all mathematical propositions are judgements a priori, Kant had thereby included propositions that we do not at present know, and that he would similarly have included as a

posteriori empirical propositions whose truth no experience has revealed to us.

In § 148 of the *Wissenschaftslehre* Bolzano had given a similarly non-epistemic definition of 'analytic'. Bolzano's classification was of *propositions* (what he calls '*Sätze an sich*'), not of sentences. This means that the work to be done by definitions, at the level of linguistic expression, has, as it were, already taken place; just as Kant spoke of a subject as 'containing' a predicate, although the predicate might not be apparent in its verbal expression, so Bolzano thought of a complex idea as containing its constituents. If we transpose from the mode of sentences and their component words to that of propositions and their component ideas, he in effect used the notion expressed by Quine as 'essential occurrence'. An analytic truth in the wider sense was for him a true proposition containing at least one idea inessentially: no admissible replacement of that idea by another would deprive the proposition of truth. An analytic truth in the narrower sense was one in which all but the logical concepts occur inessentially. Thus in § 197 he expressly observed that the two distinctions, analytic/-synthetic and conceptual/intuitive, cut across one another: there are instances of all four combinations. The proposition, 'This triangle is a figure', exemplifies the class of intuitive analytic truths, since the use of the demonstrative in its linguistic expression indicates that the idea expressed is a mixture of intuition and concept.

In classifying propositions according to their intrinsic characteristics rather than how we can know them, we do so by reference to what renders them true. Frege's later insistence that what renders them true is independent of our knowledge of them is matched by the manner in which, for example, he specifies the meaning of the universal quantifier, namely in terms of what makes a universally quantified statement true, and not at all in terms of how we can recognise it as such. It is therefore surprising that he did not at least supplement his epistemic classification by an ontic one. The explanation is surely that, with no ground for the assumption, Frege presumed that all true statements of arithmetic were provable by us. The distinction between epistemic and ontic necessity is precisely that between proof-theoretic and model-theoretic consequence. A logical formula may be called 'provable' if it is a theorem of some axiomatic formalisation, or derivable from the null set of hypotheses in a natural deduction system. A statement is analytic in Frege's sense if it is the definitional equivalent of an instance of a provable formula. If we transpose back from the mode of propositions to that of sentences, a statement is analytic in Bolzano's sense if it is the definitional equivalent of an instance of a (model-theoretically) valid formula. Had Frege recognised that there might be arithmetical truths we are incapable of proving, he would surely have accorded them such a status. Similarly, a statement will, for Frege, be synthetic a priori if it is the definitional equivalent of one deductively derivable from the fundamental non-logical laws; the corresponding ontic

notion would be that of a definitional equivalent of a statement semantically entailed by those laws.

In virtue of the completeness of first-order logic, and the incompleteness of that of second order, the epistemic and ontic notions will coincide for statements that do not involve higher-order quantification, but not for those that do. Frege of course never formulated the concept of completeness, partly because he did not really think in terms of schematic letters; what look like schematic letters in his logical notation are, officially, variables bound by tacit initial quantifiers. At any event, he never attached any particular significance to the first-order fragment of his logical theory; for him, second-order quantification was indispensable for the definitions of 'natural number' and of cardinal equivalence, and even for that of class-membership. The present notion of ontic necessity has little to do with Kripke's notion of metaphysical necessity, which relates to the behaviour of sentences when governed by modal operators interpreted non-epistemically. An example would be the statement 'It is now 4 o'clock G.M.T.', made at a moment when it was true. Since 'now' and '4 o'clock G.M.T.' are rigid designators, if it is now 4 o'clock G.M.T., there is no possible world in which it is not now 4 o'clock G.M.T.; hence it would be false to say, 'It might not have been 4 o'clock G.M.T. now', unless one meant the remark in an epistemic sense, and so the original statement was metaphysically necessary. A distinction related to these was made by Aquinas in discussing the ontological argument.⁴ The statement 'God exists', he maintained, is *per se nota*, but not *nota quoad nos*, as it would be were the ontological argument valid; we can infer to its truth only from observable, if highly general, features of the world. The epithet '*nota quoad nos*' plainly means 'knowable a priori'; whether a proposition is *per se nota*, on the other hand, presumably depends on what makes it true. The notion cannot be equated with analyticity in Bolzano's sense. It could be assimilated to that of metaphysical necessity, since no one would want to assert that there is a God, but that there might not have been – unless, again, he was speaking in an epistemic sense, meaning that, for all he formerly knew, there may not have been. It is not easy to hit on Aquinas's exact meaning, since he does not make it explicit; but he deserves credit for drawing a distinction of a kind not subsequently made, so far as I know, by anyone before Bolzano.

Definition

The most serious defect in Frege's characterisations of the concepts of analyticity and apriority lies in his failure to state the conditions under which a definition is correct. The definitions to which he allows appeal to be made in the course of that proof whose existence shows a proposition to be analytic or

⁴ *Summa Theologica*, part I, question 2, article 1.

synthetic a priori must, obviously, be correct ones; but, in *Grundlagen*, Frege simply takes it for granted that we know a correct definition when we see one. It may have been his uncertainty how to fill this lacuna that deterred him from subsequently employing the terms 'analytic' and 'a priori', or repeating his definitions of them; but the difficulty goes deeper than that, and could not be escaped merely by abstaining from the use of the term 'analytic'. In the Introduction to *Grundgesetze*, having stated it as the aim of *Grundlagen* to make it probable that arithmetic is a branch of logic, he went on to claim that

In this book this will now be vindicated by deriving the simplest laws of cardinal numbers (*Anzahlen*) by logical means alone.

As Frege observed in § 4 of *Grundlagen*, the derivation necessitated a number of definitions:

Starting from these philosophical questions, we come upon the same demand as that which has independently arisen within the domain of mathematics itself: to prove the basic propositions of arithmetic with the utmost rigour, whenever this can be done . . . If we now try to meet this demand, we very soon come upon propositions a proof of which remains impossible so long as we do not succeed in analysing the concepts that occur in them into simpler ones or in reducing them to what has greater generality. Number itself is what, above all, has either to be defined or to be recognised as indefinable. This is the problem to which this book is addressed. On its solution the decision on the nature of arithmetical laws depends.

For the proofs of the basic propositions of arithmetic to be convincing, the definitions they appeal to need to be recognised as correct.

Frege's first explicit statement of the condition for a correct definition occurs in his review of Edmund Husserl's *Philosophie der Arithmetik* of 1891. Husserl's book contained an extensive discussion of *Grundlagen*, and he sent a copy of it to Frege in the year of its publication, together with offprints of two of his articles of the same year. Frege wrote a friendly reply, expressing the hope that he 'would soon find the time to reply to your objections'. He did not find the time until 1894, just a decade after the publication of *Grundlagen*, when he published a devastating review of the book. Husserl had objected to Frege's way of defining number in *Grundlagen* that 'what this method in fact allows us to define are not the contents of the concepts of direction, shape and cardinal number, but their *extensions*'.⁵ In the review, Frege replied that:

Here a divergence is revealed between psychological logicians and mathematicians. For the former it is a matter of the sense of the words and of the ideas which they fail to distinguish from the sense; for the latter, by contrast, it concerns the subject-matter itself, the reference of the words.⁶

⁵ Chapter VII, 'Frege's Attempt'.

⁶ Pp. 319–20.

At this point Frege refers in a footnote to his essay 'Über Sinn und Bedeutung'; he of course uses the term 'idea' to mean a mental image or the like. He continues:

The objection that it is not the concept, but its extension, that is defined, actually affects all definitions in mathematics. For the mathematician, it is no more correct and no more incorrect to define a conic section as the circumference of the intersection of a plane and the surface of a right circular cone than as a plane curve whose equation with respect to rectangular co-ordinates is of degree 2. Which of these two definitions he chooses, or whether he chooses another again, is guided solely by grounds of convenience, although these expressions neither have the same sense nor evoke the same ideas.

Frege is here being very unfair to Husserl: Husserl had discovered the paradox of analysis, which was so greatly to exercise G.E. Moore, and which cannot be dismissed with such nonchalance as Frege manifests. Frege expressly denies that a correct definition need capture the sense of the expression it defines: it need only get the reference right. This criterion cannot always be readily applied: in the very case that Husserl was discussing, how is it to be determined whether Frege's definition of 'cardinal number' secured the correct reference for it? The criterion is in any case far too weak to yield any reasonable notion of analyticity, defined as in *Grundlagen*, § 3: almost any proposition could be shown to be analytic, given suitable choices of definitions for the terms involved. This makes it likely that, by the 1890s, Frege had lost interest in the status of individual propositions in favour of that of whole theories; we saw that, in the opening sentence of the main text of *Grundgesetze*, he characterised the aim of *Grundlagen* as that of showing arithmetic, in the singular, to be a branch of logic, rather than showing arithmetical truths, in the plural, to be analytic.

The alternative definitions of 'conic section', in Frege's example, are not merely co-extensive, but *provably* co-extensive. This criterion would allow us to determine that two suggested definitions were equally correct; but, if no proposition involving the term defined can be proved without appeal to a definition of it, it would never allow us to determine any definition as correct absolutely, since we could never prove the defining expression to have the same reference as that defined.

In any event, the criterion proposed in the review of Husserl was certainly not that which Frege had in mind when writing *Grundlagen*; for, although he did not state any general criterion in that book, he explicitly insisted on a condition which, on the face of it, goes beyond the demand that the definition secure the correct reference. This is that conceptual priority be respected: no expression must be defined in terms of one that is conceptually prior to it. Frege makes this explicit, in § 64, when he discusses the means of defining 'direction' in terms of 'parallel':

Admittedly, we often conceive of the matter the other way round, and many teachers define: parallel straight lines are those which have the same direction. The proposition 'If two straight lines are both parallel to a third, they are parallel to each other' can then very conveniently be proved by appeal to the analogous proposition about identity. Only the trouble is that this is to stand the true state of affairs on its head. For everything geometrical must surely be originally given in intuition. Now I ask whether anyone has an intuition of the direction of a straight line. Of a straight line, indeed; but do we distinguish in intuition the direction of the line from the straight line itself? Hardly. The concept of direction is first arrived at through a process of intellectual activity that takes its start from the intuition. On the other hand, we do have an idea of parallel straight lines.

It has been argued by Gregory Currie that these observations rest entirely upon the peculiarities of geometry, as Frege conceived of them; and certainly it is written in such a way as to suggest this. Were this so, however, the passage would be entirely beside the point. In § 64, Frege is expressly invoking what he takes to be a case analogous to that with which he is directly concerned, the definition of the notion of (cardinal) number in terms of the relation of cardinal equivalence; and he continues to discuss the matter in terms of the analogy until § 68, when he reverts to the true topic, on the assumption that the general points established for the analogy apply also to it. If what he said in § 64 depended on a feature of the analogy that differentiated it from the principal case, namely its geometrical as opposed to arithmetical character, the entire discussion would be vitiated. Frege obviously intends his readers to understand that to define 'There are just as many *F*s as *G*s' to mean 'The number of *F*s is the same as the number of *G*s' would be to stand the true state of affairs on its head in just the same way as to define 'The line *a* is parallel to the line *b*' to mean 'The direction of *a* is the same as the direction of *b*'; if not, the discussion of direction would have no relevance to the problem how number is to be defined. But the reversal of the true state of affairs, in the case of number, could have nothing to do with intuition on Frege's view, in the light of his claim to have shown that arithmetic in no way depends on intuition. Rather, Frege is here appealing to a general principle that nothing should be defined in terms of that to which it is conceptually prior.

Some twenty years later, a lecture course of Frege's, 'Logik in der Mathematik', contained a discussion of definition, and was preserved, in a version of 1914, among his surviving papers. The views here expressed differ both from those of *Grundlagen* and of the review of Husserl. Frege distinguishes between analytic definitions and what he calls 'constructive' ones; the latter are stipulative definitions, not responsible to anything, but laying down what a new word or symbol is to mean, or the sense in which an author proposes to use an existing one. The attitude Frege expresses towards these constructive definitions coincides with Russell's. From a logical standpoint, they are mere abbreviations, since the defining expression and that defined will have the

same sense: although their psychological importance may be great, 'logically considered, they are really quite inessential'.⁷ As Eva Picardi has remarked, this is a far cry from the talk in *Grundlagen* of the fruitfulness of definition.⁸

Analytic definitions, on the other hand, are those that attempt to capture the senses of existing expressions; we hear no more about such a definition's needing to be faithful only to the reference. Frege maintains, however, that 'we shall be able to assert' that the sense of the defining expression agrees with that of the term it purports to define 'only when it is immediately evident'.⁹ He is here relying on his belief in the transparency of sense: anyone who grasps the senses of two expressions must thereby know whether or not they are the same. 'How is it possible', Frege asks, 'that it should be doubtful whether a simple sign has the same sense as a complex expression, when the sense of the simple sign is known, and that of the expression can be recognised from its composition?', and answers, 'If the sense of the simple sign is really clearly grasped, it cannot be doubtful whether it coincides with the sense of the expression.'¹⁰ When it is in this way immediately evident that the analysis captures the sense already possessed by the expression analysed, it is better not to call it a 'definition', but to present it as an axiom. This will happen in very few cases, however, since very often we do not apprehend the sense of the existing term clearly, but only in a confused fashion 'as through a fog'. In such a case, Frege recommends that we should simply use our proposed analysis as a stipulative (constructive) definition of a newly introduced word or sign, and always use the latter in place of the existing term.

At first glance, one might suspect that this was the strategy he had followed in *Grundlagen*. He does not, after all, employ familiar terminology. Instead of speaking of 'the number of *F*s', he says 'the number belonging to the concept *F*'; in place of 'There are just as many *F*s as *G*s', he says 'The concept *F* is equinumerous to the concept *G*'. But, plainly, in claiming to make it probable that the truths of arithmetic are analytic, Frege did not intend merely to be asserting the analytic character of a new theory, devised by himself to mimic number theory as ordinarily understood: he obviously meant that what everyone took to be the truths of arithmetic were analytic. In proving that every natural number has a successor, for example, he had no doubt that he was proving what anyone else would have understood by the proposition: his definitions enabled him to give such a proof, but did not confer upon the words a sense in virtue of which they expressed some quite different proposition. In this, he was simply following the standard practice of mathematicians, who, in order to prove a theorem involving terms already in use, may begin by giving rigorous

⁷ *Nachgelassene Schriften*, p. 226, *Posthumous Writings*, p. 209.

⁸ See Eva Picardi, 'Frege on Definition and Logical Proof', in C. Cellucci and G. Sambin (eds.), *Temi e prospettive della logica e della filosofia della scienza contemporanee*, vol. 1, Bologna, 1988, pp. 227–30, at p. 228.

⁹ *Nachgelassene Schriften*, p. 227, *Posthumous Writings*, p. 210.

¹⁰ *Nachgelassene Schriften*, p. 228, *Posthumous Writings*, p. 211.

definitions of them, without stopping to ask after the criterion for such definitions to be correct. The reason for his use of a special jargon in *Grundlagen* was quite different. His motive was to exhibit what he had argued to be the correct logical analysis of the familiar expressions. He had stressed that it was to *concepts* that numbers attach, and statements of number relate: he therefore intended the verbal form 'the number belonging to the concept *F*' to bring this out more perspicuously than its everyday equivalent. Likewise, numerical equality was a relation between concepts, rather than objects, and the form 'The concept *F* is equinumerous to the concept *G*' presented itself as making this apparent in a way 'There are just as many *F*s as *G*s' did not. If this *was* Frege's reason for employing his jargon, he was mistaken: but, then, he was still in a state of innocence, as yet unaware of the paradoxes with which he grappled in 'Über Begriff und Gegenstand'.

It is therefore astonishing that, even thirty years later, Frege could have come so to depreciate the conceptual analyses that had formed so large a part of *Grundlagen* as to deny the very possibility of conceptual analysis save in rare and unproblematic cases. How, at the time of writing, he conceived of the definitions given in *Grundlagen*, and how we ought to conceive of them, is best left to be discussed when they have been reviewed in more detail. For the present, it is enough to be conscious that their status is a question unresolved by Frege and critical to an evaluation of his work.

CHAPTER 4

The Value of Analytic Propositions

Analytic judgements extend our knowledge

Kant underestimated the value of analytic judgements, Frege says in § 88; and in § 91 he concludes, in direct opposition to Kant, that ‘propositions that extend our knowledge may have analytic judgements as their content’. The value of analytic propositions and that of deductive inference are essentially the same; as Frege remarked apropos of arithmetical truths in § 17, on what was at that stage of the book only the hypothesis that they were derivable from logic:

Each would then contain within itself a whole series of inferences condensed for future use, and its utility would consist in our no longer needing to make the inferences singly, but being able to express the result of the whole series simultaneously.

The point of an analytic proposition, in other words, is to encapsulate an inferential subroutine which, once established, may be repeatedly appealed to without itself having to be repeated: it is not the truth of analytic propositions which is in itself important, but their service in easing our deductive transitions from synthetic truths to other synthetic truths. Frege’s contradiction of Kant’s dictum thus represents his acknowledgement of the fruitfulness of deductive inference.

Independently of whether mathematical truths are taken to be analytic or synthetic, mathematics compels us to recognise the fruitfulness of deductive inference; on whatever basis the axioms of a mathematical theory are accepted, the theorems are established by logical proofs. That deductive reasoning can yield a vast range of unexpected consequences is therefore incontrovertible: the problem is how to explain this without rendering the validity of such reasoning problematic. It is tempting to explain the validity of simple inferential steps by appeal to the thesis that a knowledge of the premisses carries with it a knowledge of the conclusion. But, if we have already taken every step in the direction of Rome, we must already be in Rome. If the thesis were true, we

should already know all consequences attainable by a sequence of such simple steps, however long; when the theory was a first-order one, this would mean all consequences whatever. As Frege remarked in § 88, the conclusions are contained in the premisses, not as rafters within a house, but as the plant within the seed.

The solution necessarily lies in drawing an appropriate distinction between form and content. All conceptual thought involves the apprehension of pattern: a report of current observation singles out particular features from a multifarious field of perception, subsuming them under general concepts. Some patterns force themselves upon us, but others need to be discerned. The characteristic of a pattern is that it is there to be discerned, but that, to apprehend that in which it is a pattern, we do not need to discern the pattern; it is essential to our discerning the pattern that we recognise that that in which we have discerned it remains unaltered. One can hear a poem without identifying the metre or the rhyme scheme; someone unfamiliar with the Fibonacci sequence may fail to detect the principle determining the terms. When we become conscious of the metre or of the rule of generation, we perceive that pattern *in* the poem or the sequence, which we recognise as still the same poem or sequence as before.

In the present case, we are concerned, not with that imposition of pattern upon heterogeneous reality that constitutes conceptual thought, but with the discernment of pattern at a level one higher, namely in the thought itself. We may grasp the content of certain propositions, and recognise their truth; but, even when we think of them at the same time, we may well not perceive the pattern revealed by a proof of which they are the premisses. We cannot, in general, say that a verification of the premisses constitutes a verification of the conclusion. An even number is perfect just in case it is of the form $2^{n-1}(2^n - 1)$, where the odd factor is prime. The processes of verifying that it has the latter form and that it is perfect are different. The proof consists of a method of arranging the two processes simultaneously so that a falsification of either can be made to yield a falsification of the other; the possibility of such an arrangement depends on the fact that even the verification of such simple propositions consists in a sequence of steps of which the order is indifferent. To hit on the proof requires an apprehension of the pattern that makes such an arrangement possible.

Similarly with the problem of the bridges at Königsberg. The major premiss is the fact that a traveller crossed every bridge; the minor premisses are the disposition of the bridges, and the fact that he traversed a continuous path; and the conclusion is that he crossed at least one bridge at least twice. A verification of the premisses would not, in general, involve verifying the conclusion. An observer might be stationed at every bridge, noting if the traveller crossed it, and then going away; his continuous path might have been checked by someone tailing him without noticing when he crossed a bridge. The

conclusion might be verified by again stationing an observer at every bridge; as soon as one of them observes the traveller crossing his bridge a second time, he reports to base and the observations are abandoned. The proof consists in a manner of arranging any sufficiently detailed observations of the traveller's path in such a way as to verify simultaneously that he crossed every bridge and that he crossed one of them twice. Here it is not a matter merely of arranging any verifications of premisses and conclusion, but of arranging a process that simultaneously verifies all three premisses, and one that simultaneously verifies the conclusion and the minor premisses; and, again, the proof consists in the apprehension of a pattern permitting a comparative arrangement of the two processes.

Frege believed, however, that every proof could be broken down into extremely small steps, as taken in his formalised system; and we know that, as far as first-order inferences are concerned, he was demonstrably right. It was therefore necessary to solve the problem of the fruitfulness of deductive inference, not at the level of entire proofs, but at that of the simplest single steps. Frege's solution involved precisely the idea of discerning a pattern within a thought, or, rather, in the terminology of the early period, a judgeable content, a pattern it shared with a certain range of other thoughts or contents. This was the process that led him to declare in 'Booles rechnende Logik' that:¹

Instead of putting a judgement together out of an individual as subject and a previously formed concept as predicate, we conversely arrive at the concept by dissecting the judgeable content.

Why should he say this? If the judgeable content is complex, why should we prefer the metaphor of dissecting it so as to extract the constituents to that of putting it together out of those constituents? It is not, as some have thought, that Frege had some strange idea of our apprehending the judgeable content, in the first instance, as a simple unit devoid of complexity: he scotches that interpretation in the very next sentence after that quoted above:

Admittedly, in order to be able to be so dissected, the expression of the judgeable content must already be composite.

The reason is, rather, that the metaphor of 'putting together' is appropriate to that complexity which we must apprehend in order to grasp the content at all. It is impossible to grasp the thought expressed by 'Either Venus is larger than Mars or Mars is larger than Mercury' save as a disjunction of two simpler thoughts; it is impossible to grasp that expressed by 'The Earth rotates' save as predicating something of the object denoted by 'the Earth'. That is why

¹ *Nachgelassene Schriften*, p. 18, *Posthumous Writings*, p. 17.

'the Earth' expresses a genuine component of the latter thought, and why the connective 'Either ... or ...', and the two subsentences, all express genuine components of the former; it is of such examples that Frege stated, in his middle period, that the sense of a part of a sentence is a part of the thought expressed by the whole. But the process of dissection referred to in the passage from 'Booles rechnende Logik' is not, in general, aimed at extracting such components: it is a process of *concept-formation*, aimed at arriving at something new, which is why he had said, in the previous paragraph, 'I admit the formation of concepts as arising first from judgements'.

The process is described in *Begriffsschrift*, § 9, as follows:

If we suppose that the circumstance that hydrogen is lighter than carbon dioxide is expressed in our formalised language, we can replace the symbol for hydrogen by the symbol for oxygen or for nitrogen. By this means, the sense is altered in such a way that 'oxygen' or 'nitrogen' enters into the relations in which 'hydrogen' formerly stood. By thinking of an expression as variable in this manner, it is dissected into a constant component, which represents the totality of the relations, and the symbol which is thought of as replaceable by another, and which signifies the object that stands in those relations. I call the former constituent the function, the latter the argument.

The same sentence or judgeable content can be dissected in different ways; a simple example used by Frege is the proposition that Cato killed Cato:

If we think of 'Cato' as replaceable at the first occurrence, the function is 'to kill Cato'; if we think of 'Cato' as replaceable at the second occurrence, the function is 'to be killed by Cato'; finally, if we think of 'Cato' as replaceable at both occurrences, the function is 'to kill oneself'.

The process is succinctly explained, in essentially the same way, in 'Booles rechnende Logik'; Frege there uses the variable '*x*' to indicate the effect of treating a given term as replaceable by others. If, in the equation

$$2^4 = 16$$

we treat the '2' as replaceable, we obtain the concept '4th root of 16'; if we treat the '4' as replaceable we obtain the concept 'logarithm of 16 to the base 2'. The talk of imagining a term as replaceable by others shows that the constant part – what in *Begriffsschrift*, but hardly at all in later writings, Frege called 'the function' – constitutes a pattern common to all the sentences obtained by making such a replacement.

In *Grundlagen*, the process of dissection is directly referred to only in § 70, where Frege uses it to explain his general notion of what he here calls a 'relation-concept'. He does not now use the psychological language of imagining a term as replaceable by others, but speaks of 'subtracting' it: what remains, when we subtract one term, is an expression for a concept, and, when we

subtract two, one for a relation-concept; but, since either 'demands a completion to make a judgeable content', the two metaphors have exactly the same application.

This was not in fact the only process of concept-formation Frege was prepared to admit. The process by which we attain to such concepts as shape, direction and number itself, exhaustively described in §§ 63–9 of *Grundlagen*, is quite different; and in § 34 he seems prepared to allow that *some* concepts can be attained by the process of abstraction. The concepts Frege believed to be attainable only through judgements or complete propositions were those expressed, in the first instance, by *complex* predicates, to any of which we may, in interesting cases, equate some newly introduced simple predicate by definition. The reason why such a concept has to be regarded as attained, not by being built up out of its constituents, but by the dissection of a proposition, is that, on Frege's view, the sense of a complex predicate is not directly derivable from its components. From the proposition 'Either Jupiter is larger than Neptune and Neptune is larger than Mars, or Mars is larger than Neptune and Neptune is larger than Jupiter', we can extract the predicate 'Either Jupiter is larger than x and x is larger than Mars, or Mars is larger than x and x is larger than Jupiter', thus attaining the concept 'intermediate in size between Jupiter and Mars'. But neither the connective 'or' nor the connective 'and', if regarded as primitive, is to be explained as operating on two predicates to form a new complex predicate: each is explained only for the case in which it serves as the principal operator in a complete proposition. Hence the complex predicate cannot be understood save as extractable from such a proposition as that cited above: its sense may be seen as being given as a function carrying the sense of the name 'Neptune' on to the thought expressed by 'Either Jupiter is larger than Neptune and Neptune is larger than Mars, or Mars is larger than Neptune and Neptune is larger than Jupiter', the sense of the name 'Venus' on to the thought expressed by 'Either Jupiter is larger than Venus and Venus is larger than Mars, or Mars is larger than Venus and Venus is larger than Jupiter', and so on. We can regard it as such a function only because we *already* understand the complete propositions; it is in grasping *their* contents that we directly advert to the meanings of the connectives 'or' and 'and'.

The process of dissection thus does not respect that structure in virtue of which we grasp the content of the proposition in accordance with its composition; what it yields is, in general, a feature which the proposition shares with others, but of which we did not have to be aware in order to grasp its content. To understand the proposition 'Jupiter is larger than Neptune and Neptune is larger than Mars', it is not necessary so much as to notice that the name 'Neptune' occurs in both subsentences, let alone to conceive of the range of propositions obtainable by replacing it in both occurrences by some other name: all that is necessary is to understand both subsentences and the meaning

of 'and'. The point is stated clearly in *Begriffsschrift*, § 9, where, immediately after the sentence quoted above explaining his use of the terms 'function' and 'argument', Frege says:

This distinction has nothing to do with the conceptual content, but is only a matter of how we regard it.

Dissection is therefore justly described as a process of concept-formation: it reveals something new, one pattern among many discernible in the proposition and shared by it with others, but not, in general, intrinsic to a grasp of its content.

It is when they essentially involve the process of dissection that, in *Grundlagen*, Frege regards definitions as fruitful. As he says in § 88:

[Kant] seems to think of a concept as determined by co-ordinate characteristics; but this is one of the least fruitful methods of concept-formation. Anyone who surveys the definitions given above will scarcely find one of this kind. The same holds of the truly fruitful definitions of mathematics, for example that of the continuity of a function. In these we do not have a sequence of co-ordinate characteristics, but a more intimate – I should like to say, more organic – combination of specifications. The distinction can be made intuitive by means of a geometrical picture. If one represents the concepts (or their extensions) by regions of a plane, what corresponds to a concept defined by means of co-ordinate characteristics is the region common to all the regions representing those characteristics; it is enclosed by segments of their peripheries. In giving such a definition, therefore, it is a matter – to speak pictorially – of using the already given lines in a new way to delimit a region. Nothing essentially new emerges from this.

As Frege remarks in a footnote, the case is similar when the characteristics are connected by disjunction. He continues:

The more fruitful determinations of concepts draw boundary lines which were not previously given at all. What we shall be able to infer from them cannot be predicted in advance; we are not in this case simply taking out of the chest what we had put into it.

And from this he draws the conclusion that 'the consequences derived advance our knowledge'.

This, then, is Frege's explanation of the fruitfulness, not merely of definition, but of deductive reasoning, and, with it, of analytic propositions. But why does he link the two? The reason is that dissection is necessary in order to recognise the validity of inferences. If we define ' x is intermediate in size between y and z ' to mean 'Either y is larger than x and x is larger than z , or z is larger than x and x is larger than y ', we need, if we are to draw the conclusion 'There is a body intermediate in size between Jupiter and Mars', to be able to recognise the complex three-place predicate as extractable from the proposition 'Either

Jupiter is larger than Neptune and Neptune is larger than Mars, or Mars is larger than Neptune and Neptune is larger than Jupiter': we have to discern that pattern in it. This does not apply only when a definition is involved: since it is, in general, to a complex predicate that a quantifier is attached in order to form a quantified proposition, the operation of dissection must be conceived as a necessary preliminary to the formation of a quantified proposition in the standard case. In order to frame the proposition 'For some x , Jupiter is larger than x and x is larger than Mars', the complex predicate 'Jupiter is larger than x and x is larger than Mars' has first to be extracted from such a proposition as 'Jupiter is larger than Venus and Venus is larger than Mars'. This predicate is not a component of the proposition from which it was extracted by dissection, in that we do not have to recognise its presence in order to grasp the content of the proposition; but it *is* a component of the quantified proposition. As Frege puts it, clumsily but clearly, in § 9 of *Begriffsschrift*:

When the argument is *indeterminate*, as in the judgement, 'You can take an arbitrary positive integer as argument for "to be representable as the sum of four squares"', and the proposition will always remain correct', the distinction between function and argument becomes of significance as regards the *content*.

Deductive reasoning is thus in no way a mechanical process, though it may be set out so as to be checkable mechanically: it has a creative component, involving the apprehension of patterns within the thoughts expressed, and relating them to one another, that are not required for or given with a grasp of those thoughts themselves. Since it has this creative component, a knowledge of the premisses of an inferential step does not entail a knowledge of the conclusion, even when we attend to them simultaneously; and so deductive reasoning can yield new knowledge. Since the relevant patterns need to be discerned, such reasoning is fruitful; but, since they are there to be discerned, its validity is not called in question.

Such was Frege's solution to the problem of the utility of deductive reasoning. He is one of the very few to have faced the problem at all: J.S. Mill was another, but his solution failed completely. Whether or not the specific explanation that Frege offered is adequate, it is surely along the right general lines. All conceptual thought involves the imposition of form upon an amorphous reality: on Frege's account, deductive reasoning requires the further imposition of form upon our thoughts. It is surely that conception that can alone explain how such reasoning can be at the same time fruitful and cogent in virtue solely of the contents of the thoughts involved.

Ranges of application

It would be a mistake, though a natural one, to suppose that Frege's only ground for maintaining the truths of arithmetic to be analytic was his detailed reduction of its fundamental laws to logical truths: for he has, besides, some general arguments, based on the universal applicability he ascribes to arithmetic. *Grundlagen* in fact advances two distinguishable theses about arithmetical truths: that they are analytic, and that they are expressible in purely logical terms. On his own principles, neither implies the other. The presence of non-logical expressions in a formulation of the axioms of geometry does not, of itself, prove those axioms to be synthetic; for there might be some system of definitions connecting the geometrical terms the application of which would render them derivable from logical first principles. Conversely, a synthetic proposition might be expressible by means of logical notions alone. An example would be Russell's Axiom of Infinity, which says that there are infinitely many individuals: since, for Russell, neither numbers nor classes – what Frege regarded as logical objects – are individuals, the analytic character of this axiom can hardly be sustained. For a proposition to be analytic in Frege's sense, it must follow from the fundamental laws of logic, which neither need nor admit of proof. These laws are ones we recognise, and must recognise if we are to be able to reason: but Russell's Axiom of Infinity is neither included among these nor derivable from them. Indeed, it is very probably untrue.

Propositions differ, on Frege's view, according to their range of applicability; the extent of that range is to be measured along two dimensions, corresponding to the two features just considered: the modal status of a proposition, as a posteriori, synthetic a priori or analytic, and the vocabulary needed for its expression. The second of these two dimensions relates to the region of reality within which the proposition holds good: it may be true of material objects only, or, more generally, of spatio-temporal objects, or of all objects whatsoever. The other dimension relates, rather, to the degree of reality: the proposition may be true of everything there actually is, or of everything we can imagine, or of everything of which we can intelligibly think at all.

Arithmetical propositions, Frege argued, have maximal applicability along both dimensions. They apply to all regions of reality: objects of every type can be counted. More exactly expressed, we may ask, of objects of every type, how many there are satisfying some given condition. The point is made in *Grundlagen*, § 24, where it is used to refute the empiricist view that number is a physical property; but its full implications are drawn in the lecture 'Über formale Theorien der Arithmetik' given by Frege in 1885, the year after the book's publication, which starts characteristically as follows:

Under the name 'formal theory' I wish here to consider two conceptions, of which I agree with the first, but seek to controvert the second. The first says that all arithmetical propositions can be derived purely logically from definitions alone,

and consequently must be so derived. . . . Of all the grounds that tell in favour of this view, I wish here to cite only one, that which rests upon the all-embracing applicability of arithmetical theorems. Virtually everything that can be an object of thought may in fact be counted: the ideal as well as the real, concepts as well as things, the temporal as well as the spatial, events as well as bodies, methods as well as theorems; even the numbers themselves can in turn be counted. Nothing is really demanded save a certain sharpness of circumscription, a certain logical completeness. From that fact can be gathered this much, that the fundamental principles on which arithmetic is constructed cannot relate to a narrower domain whose peculiarities they express as the axioms of geometry express those of what is spatial. Rather, those fundamental principles must extend to everything thinkable; and a proposition that is in this way of the greatest generality is justifiably assigned to logic.

The argument does not show that arithmetical terms and concepts can be *reduced* to logical ones. Rather, it shows that they are *already* logical in character. The only differentiation of logical notions from others ever considered by Frege rested on their being unrestricted in the subject-matter to which they could be applied, rather than being confined to any particular domain of knowledge. Once it is recognised that there is no segment of reality composed of objects that cannot be numbered, it is thereby recognised that the notion of number is a logical one. By itself, this as yet says nothing about the grounds on which we accept the laws of arithmetic as true; it tells us only that they are expressible in purely logical terms, or, rather, that they are already stated in purely logical terms. The definitions given in *Grundlagen* of arithmetical notions in terms of simpler ones are required more in order to make manifest the grounds of those laws than to establish that they are logical in nature.

The laws of arithmetic have maximal generality in the other dimension also: they apply to all that can be grasped by conceptual thought. The argument in this case concerns the ground of our knowledge. Just as he was later to do in the essay 'Erkenntnisquellen der Mathematik und der mathematischen Naturwissenschaften' which he wrote for publication in the last year of his life, Frege operated with a threefold classification of grounds of knowledge: observation; spatial and temporal intuition; and our logical faculty. Observation can tell us only how things *actually* are. Something we observe to be so may be true of all that is imaginable, or even of all that is conceivable; but, if we know it by observing it to be so, we have no ground to suppose that it holds good of more than what there actually is. A priori spatial or temporal intuition tells us of how things must be if we are either to apprehend or to imagine them as in space or time; it cannot tell us of how they must be even if we are unable to apprehend or imagine them. Only of what we know in virtue of our unaided logical faculty do we have any ground for supposing it to hold good of everything thinkable.

It follows conversely that, if we find it impossible to imagine the contrary of some general law, we have probably derived it from a priori intuition; and,

if we find it impossible even to conceive of the contrary as an intelligible possibility, we have probably come to know it by the use of our logical faculty. The argument was most cogently set out by Frege in one of *Grundlagen's* purple passages, § 14, which is worth quoting in full; it must be borne in mind, in reading it, that the phrase 'the axioms of geometry' meant, for Frege, 'the axioms of *Euclidean* geometry'.

A comparison of truths in respect of the domains which they govern also tells against the empirical and synthetic nature of arithmetical laws.

Empirical propositions hold of physical or psychological actuality, while geometrical truths govern the domain of the spatially intuitable, whether actual or the product of our imagination. The maddest fantasies of delirium, the most daring inventions of legend or of the poets, which have animals speaking and the stars standing still, which make men from stones and trees from men, and teach how one can pull oneself out of a swamp by one's own forelock, are yet subject to the axioms of geometry, as long as they remain intuitable. Only conceptual thought can in a certain fashion shake free of those axioms, when it assumes a space of four dimensions, say, or of positive curvature. Such considerations are not in the least useless; but they completely abandon the base of intuition. If we do call intuition to our aid in this connection, it is still the intuition of Euclidean space, of the only space of whose structure we have any intuition. It is then taken, not for what it is in itself, but as symbolic for something else; for example, we call something a straight line or a plane which we perceive as curved. For conceptual thought we can always assume the opposite of this or that geometrical axiom, without involving ourselves in any self-contradictions when we draw deductive consequences from assumptions conflicting with intuition such as these. This possibility shows that the axioms of geometry are independent of one another and of the fundamental laws of logic, and are therefore synthetic. Can one say the same of the fundamental principles of the science of number? Does not everything collapse in confusion when we try denying one of them? Would thought itself then be possible? Does not the ground of arithmetic lie deeper than that of all empirical knowledge, deeper even than that of geometry? The truths of arithmetic govern the domain of what is countable. This is the most comprehensive of all; for it is not only what is actual, not only what is intuitable, that belongs to it, but everything thinkable. Should not the laws of number then stand in the most intimate connection with those of thought?

That the axioms of geometry can be denied without contradiction does not *prove* that they are synthetic: it is what is *meant* by saying that they are synthetic. But, as Frege frequently pointed out in other connections (and as he was to discover to his bitter cost), the fact that we have not come upon a contradiction does not prove that none is lurking. Until a consistency proof was available, no more could be said than that our not having encountered a contradiction suggested that there was none, and hence that the axioms of Euclidean geometry were synthetic. Likewise, our inability to describe coherently a state of affairs in which any of the laws of arithmetic failed does not demonstrate that they are analytic: a proof that they stand or fall with the laws of logic,

such as Frege attempted to give, is needed for that. The argument from the applicability of arithmetic to everything that can be grasped by conceptual thought was no more than suasive. It remains that Frege had to hand quite a powerful suasive argument in favour of the thesis he wished to establish: for it at least appears that we can make no intelligible sense, of the kind we can make of a denial of the parallel postulate, of the supposition that the laws of arithmetic might not hold.

CHAPTER 5

Frege and Dedekind

Dedekind's *Was sind und was sollen die Zahlen?* appeared four years after Frege's *Grundlagen*, but was certainly composed independently of it; in his Preface, Dedekind states that a first draft of his book was completed by 1878 and privately circulated during the ensuing decade. In the Preface to the second edition, of 1893, he paid an extended tribute to Frege's book; it was ironic that, in the Preface to *Grundgesetze*, published only a month earlier, Frege, calling Dedekind's book 'the most profound work on the foundations of arithmetic that has lately come to my notice', had complained that, among others, its author appeared to be unacquainted with his own work.¹ It hardly detracts from the originality of Dedekind's book to observe that it owes much to Bolzano's *Paradoxien des Unendlichen*, which he acknowledges; Frege, on the other hand, appears to have known none of Bolzano's writings.

Of the two, Frege's book was by far the more philosophically pregnant and perspicacious; but there is a clear sense in which Dedekind's revealed much more about the natural numbers. Dedekind was the first to state and justify the general principles governing the definition of a function by recursion, which he formulated for one whose values need not be natural numbers.² He used recursion to define addition, multiplication and exponentiation,³ and proved the fundamental algebraic laws holding for them. A reader of *Grundlagen*, on the other hand, who has kept in mind the sustained discussion of numerical equations in §§ 5 to 17, may be surprised to discover the book coming to an end before addition has even been defined. *Grundlagen* purports to make it probable that the truths of arithmetic are analytic; yet those whose proofs are given or sketched do not include what, in § 2, Frege had called 'the simplest propositions holding of the positive integers, which form the foundation of the whole of arithmetic'. The addition of cardinal numbers is, admittedly, touched on at the end of Part II of *Grundgesetze*;⁴ but even there

¹ *Grundgesetze*, vol. I, p. viii and p. x, fn. 1.

² *Was sind und was sollen die Zahlen?*, § 9.

³ *Ibid.*, §§ 11–13.

⁴ Vol. II, §§ 33–44.

it is not systematically investigated, and multiplication is never treated of at all. Frege acknowledged that Dedekind had carried his derivation of the laws of arithmetic a great deal further than he himself had done, but explained that this was possible for him because he was not interested, like Frege, in giving formal proofs that exclude the possibility of oversight and render us fully conscious of everything involved in them.⁵

Dedekind's approach to the question posed in his title differs utterly from Frege's. Dedekind tackled it more specifically in the spirit of a mathematician, Frege more in that of a philosopher; Dedekind's treatment was that of a pure mathematician, whereas Frege was concerned with applications. Dedekind's central concern was to characterise the abstract structure of the system of natural numbers; what those numbers are used for was for him a secondary matter. In this respect Frege, pioneer as he was, was old-fashioned. From § 18 to § 83 of *Grundlagen*, he occupies himself exclusively with the question, 'What is number?', and its ancillary, 'What are the individual numbers such as 0 and 1?'. Up to § 44, he reviews and criticises the answers of Mill, Kant and many other philosophers and mathematicians. All of these take for granted that, to say what number is, we must simultaneously explain what numerical equations and the like are about, and analyse the use of number-words in empirical contexts to answer questions beginning 'How many . . . ?'. Frege does not challenge this assumption: he shares it. Kant took it for granted that the symbol '5' in the equation $5 + 7 = 12$ has an immediate connection with the word 'five' as it occurs in 'I have five fingers on my left hand'; and Frege took it for granted, too. For both of them, arithmetical propositions are about numbers in the same sense of the word 'number' as that in which we speak of the number of Jupiter's moons or of inhabitants of Berlin.

Even the German language helped Frege to make this assumption appear inescapable. In the footnote to § 4, he explained that he would be almost exclusively concerned with 'the positive integers, which answer the question, "How many?"', though, on his own principles, he ought to have said 'the non-negative integers'. In speaking of them, he usually employs the word '*Anzahl*' rather than '*Zahl*'. As his English translator, Austin, notes, '*Anzahl*' has the sense of 'cardinal number', but not its technical ring, being a quite everyday word; Austin is therefore driven to distinguishing '*Anzahl*' from '*Zahl*' by writing 'Number' with a capital letter. Frege needed some verbal means of distinguishing the natural numbers from rationals, real numbers, etc.; his choice of the word '*Anzahl*' for this purpose was powerful subliminal propaganda for the view that their essential characteristic is their use as finite cardinals.

Dedekind, by contrast, relegated that use of them to a wholly subordinate status. In his book, he did not characterise the natural number system by

⁵ *Grundgesetze*, vol. I, pp. vii–viii.

axiomatising number theory, although in fact what are known as the Peano axioms were first enunciated by him in private correspondence.⁶ Instead, he gives a direct characterisation of structures that serve as models for the Peano axioms, calling them 'simply infinite systems', which are what Russell later called 'progressions'; the four conditions in the definition of a simply infinite system correspond closely to the Peano axioms.⁷ He feels obliged then to prove that the class of simply infinite systems is not empty. This he does by a piece of non-mathematical reasoning; his example is the system whose initial element is my self (*mein eigenes Ich*) and which is generated by the operation that carries an object x into the thought that x can be an object of my thinking.⁸

He now comes, by a means not at all to Frege's taste, to define the natural numbers. Dedekind's philosophy of mathematics was that mathematical objects are 'free creations of the human mind', as he says in the Preface. He neither amplified nor defended this belief; but he adhered tenaciously to it. The idea, widely shared by his contemporaries, was that abstract objects are actually created by operations of our minds. This would seem to lead to a solipsistic conception of mathematics; but it is implicit in this conception that each subject is entitled to feel assured that what he creates by means of his own mental operations will coincide, at least in its properties, with what others have created by means of analogous operations. For Frege, such an assurance would be without foundation: for him, the contents of our minds are wholly subjective; since there is no means of comparing them, I cannot know whether or not my idea is the same as yours. Even if this could be known, there could be no ground for declaring one person right and the other wrong, if their ideas proved to be different: as Frege says in the Preface to *Grundgesetze*:⁹

It is impossible to ascribe to each person his own number one; for it would then have first to be investigated how far the properties of these ones coincided. And if one person said, 'Once one is one', and another, 'Once one is two', we could only register the difference and say: your one has that property, mine has this.

That is why thoughts, or judgeable contents, which are communicable and can be judged by anybody true or false absolutely, rather than true for one person and false for another, are not to be viewed as contents of the mind: as he wrote in the 'Logik' of the 1880s:¹⁰

A judgeable content . . . is . . . not the result of an inner process or the product of some human being's mental operation, but something objective, which means

⁶ Hao Wang, 'The Axiomatisation of Arithmetic', *Journal of Symbolic Logic*, vol. 22, 1957, pp. 145–57.

⁷ *Was sind und was sollen die Zahlen?*, § 6, definition 71.

⁸ § 5, theorem 66.

⁹ P. xviii.

¹⁰ *Nachgelassene Schriften*, p. 7, *Posthumous Writings*, p. 7.

something that is exactly the same for all rational beings, for all capable of grasping it, just as the Sun, say, is something objective.

One of the mental operations most frequently credited with creative powers was that of abstracting from particular features of some object or system of objects, that is, ceasing to take any account of them. It was virtually an orthodoxy, subscribed to by many philosophers and mathematicians, including Husserl and Cantor, that the mind could, by this means, create an object or system of objects lacking the features abstracted from, but not possessing any others in their place. It was to this operation that Dedekind appealed in order to explain what the natural numbers are. His procedure differed from the usual one. Husserl, in company with many others, supposed that each individual cardinal number was created by a special act of abstraction: starting with any arbitrary set having that number of members, we abstract from all the properties possessed by the individual members of the set, thus transforming them into featureless units; the set comprising these units was then the relevant cardinal number. Cantor's variation on this account was a trifle more complex: we start with an ordered set, and abstract from all the features of the individual members, but not from their ordering, and thus obtain their order-type; next, we abstract from the ordering relation, and obtain the cardinal number as an unordered set of featureless units, as before. Frege devoted a lengthy section of *Grundlagen*, §§ 29–44, to a detailed and conclusive critique of this misbegotten theory; it was a bitter disappointment to him that it had not the slightest effect. Cantor, who might have been supposed to have read *Grundlagen*, since he reviewed it, persisted undeterred with his abstractionist account;¹¹ Husserl, in his book of 1891, again subscribed to it, despite his lengthy discussion of *Grundlagen*.

Dedekind, on the other hand, applies the operation of abstraction to an arbitrary simply infinite system to obtain from it the system of natural numbers.¹²

If, in considering a simply infinite system N , ordered by a mapping ϕ , we entirely disregard the particular nature of its elements, retaining only their discriminability from each other, and having regard only to the relations to one another imposed by the mapping ϕ which orders them, then these elements are called *natural numbers* or *ordinal numbers* or simply *numbers*.

The mapping ϕ is of course the operation that generates the system, corresponding to the successor function.

Having thus defined the natural numbers, Dedekind develops the theory of

¹¹ Michael Hallett, in his superb study, *Cantorian Set Theory and Limitation of Size*, Oxford, 1984, squarely faces the difficulties with Cantor's version of the theory, pp. 128–33, and then boldly attempts a defence of it, pp. 133–42, but not, to my mind, successfully.

¹² § 6, definition 73.

them in §§ 7–13: only in the final section, § 14, does he give an account of the use of the natural numbers to give the cardinality of finite systems, by using the same notion of one-one correlation employed by Frege in *Grundlagen* and by Cantor in papers from 1874 onwards. In complete contrast to Frege's method of defining the natural numbers, this application of them is not central to Dedekind's way of characterising them; it is external, an appendage which could have been omitted without damaging the theory as a whole. This divergence is reflected in the way each defines the sum of two natural numbers. Dedekind does so by means of the recursion equations for addition; Frege, in effect, as the number of members of the union of two disjoint classes. Dedekind indeed proves such a union to have $m + n$ members if the two classes had m and n members respectively (§ 14, theorem 168); but, for him, it required proof, rather than being immediate from the definition, and was a mere addendum to his general treatment of addition in § 11.

Frege and Dedekind were at odds over two interconnected questions: whether or not the use of natural numbers to give the cardinality of finite totalities is one of their distinguishing characteristics, which ought therefore to figure in their definition; and whether it is possible, not merely to characterise the abstract structure of the system of natural numbers, but to identify the natural numbers solely in terms of that structure. Unlike Frege's, Dedekind's natural numbers have no properties other than their positions in the ordering determined by their generating operation, and those derivable from them; the question is whether such a conception is coherent.

In his great book of 1903, Bertrand Russell criticised Dedekind's theory as follows:¹³

It is impossible that the ordinals should be, as Dedekind suggests, nothing but the terms of such relations as constitute a progression. If they are to be anything at all, they must be intrinsically something; they must differ from other entities as points from instants, or colours from sounds. . . . Dedekind does not show us what it is that all progressions have in common, nor give any reason for supposing it to be the ordinal numbers. . . . What Dedekind presents to us is not the numbers, but any progression: what he says is true of all progressions alike, and his demonstrations nowhere . . . involve any property distinguishing numbers from other progressions. No evidence is brought forward to show that numbers are prior to other progressions. We are told, indeed, that they are what all progressions have in common; but no reason is given for thinking that progressions have anything in common beyond the properties assigned in the definition, which do not themselves constitute a new progression.

Russell is here obstinately refusing to recognise the role assigned by Dedekind to the process of abstraction. He thinks that, if we are to refer to 'the' natural numbers, or 'the' finite ordinals, we must thereby refer to quite specific

¹³ *The Principles of Mathematics*, p. 249.

objects; but Dedekind would not deny this. He thinks, further, that if these numbers are to be specific objects, they must possess properties other than the purely structural ones they have in virtue of their positions in the sequence; but that is just what Dedekind would deny. He believed that the magical operation of abstraction can provide us with specific objects having only structural properties: Russell did not understand that belief because, very rightly, he had no faith in abstraction thus understood.¹⁴

Mathematicians frequently speak as if they did believe in such an operation. One may speak, for example, of 'the' five-element non-modular lattice. There are, of course, many non-modular lattices with five elements, all isomorphic to one another: if you ask him which of these he means, he will reply, 'I was speaking of the abstract five-element non-modular lattice'. But, even if he retains a lingering belief in the operation of abstraction, his way of speaking is harmless: he is merely saying what holds good of *any* five-element non-modular lattice. That is how neo-Dedekindians such as Paul Benacerraf, who have understandably jettisoned the doctrine of abstraction, would have us suppose it to be with the natural numbers.

The system of natural numbers differs from the lattice in that, for many of their applications, for instance in giving a definition by induction upon them, it is essential to know that such a system exists. Dedekind recognised that necessity. If he had proved the existence of a simply infinite system by purely mathematical means, for instance from the theory of classes, could he not have identified the system so proved to exist with that of the natural numbers? Or was it essential that the system of natural numbers be what all simply infinite systems have in common?

Some 'mathematical objects' really have only a pure structural identification, and thus, as Benacerraf argues concerning the natural numbers, are not genuine objects at all: there is no more such an object as the zero of 'the' eight-element Boolean algebra than there is such a point as the centre of 'the' circle with unit radius. As far as the natural numbers are concerned, however, Frege and Russell are right, and Dedekind and Benacerraf wrong: we take them as too intimately connected with certain immediate applications of them to regard them as identifiable solely through the internal structure of the natural-number system. Benacerraf writes:¹⁵

Any object can *play the role of 3*; that is, any object can be the third element in some progression. What is peculiar to 3 is that it defines that role – not by being

¹⁴ Russell's interpretation makes Dedekind's proof of the existence of a simply infinite system inessential: number theory is what holds good of all simply infinite systems; to dispel the fear that the theory might be idle, we observe that there is at least one such system, on the contrary, the proof was essential for Dedekind: without a simply infinitive system to which to apply the operation of abstraction, we could not arrive at the natural numbers.

¹⁵ 'What Numbers Could Not Be', *Philosophical Review*, vol. LXXIV, 1965, p. 70.

a paradigm of any object which plays it, but by representing the relation that any third member of a progression bears to the rest of the progression.

But whether 3 is the third or the fourth term in the sequence of natural numbers depends whether you start with 1 or with 0. Frege started with 0, because 0 is needed as a finite cardinal; Dedekind started with 1, for no especial reason; Husserl, eccentrically, started with 2, on the ground that neither 0 nor 1 is a number. The number 0 is not differentiated from the number 1 by its position in a progression, otherwise there would be no difference between starting with 0 and starting with 1. That is enough to show that we do not regard the natural numbers as identifiable solely by their positions within the structure comprising them.

It might be retorted that this objection depends upon a mere tactical mistake on Benacerraf's part: if he had considered the structure $\langle N, 0, S, +, . \rangle$ or $\langle N, 1, S, +, . \rangle$ instead of $\langle N, 0, S \rangle$ or $\langle N, 1, S \rangle$, the problem would not have arisen. It is not a mere question of tactics, however: if he had done that, he would have been false to his own principles. If we are concerned, as Benacerraf is, with what it is that mathematicians are talking about, we have to think, not merely of mathematical structures, but of how they are given to us, that is, how they are characterised. Obviously, Benacerraf must have in mind a second-order characterisation, which alone yields a categorical specification of the structure of the natural numbers. Under a second-order characterisation, however, there is no call to treat addition and multiplication as primitive, since they are definable; the structure is completely determined by the Peano axioms, and needs no further determination in terms of other operations. Benacerraf's thesis is that structure is all that matters, since we can specify a mathematical object only in terms of its position in the structure to which it belongs. The thesis is false, and the example Benacerraf chose to illustrate it is the very one that most clearly illustrates its falsity. The identity of a mathematical object may sometimes be fixed by its relation to what lies outside the structure to which it belongs; what is constitutive of the number 3 is not its position in any progression whatever, or even in some particular progression, nor yet the result of adding 3 to another number, or of multiplying it by 3, but something more fundamental than any of these: the fact that, if certain objects are counted 'One, two, three', or, equally, 'Nought, one, two', then there are 3 of them. The point is so simple that it needs a sophisticated intellect to overlook it; and it shows Frege to have been right, as against Dedekind, to have made the use of the natural numbers as finite cardinals intrinsic to their characterisation. We shall see later that this represents, not a trifling detail, but a fundamental principle, of his philosophy of arithmetic.

For all that, the thesis that Benacerraf is principally concerned to oppose, that the natural numbers are quite specific objects, with which any correct analysis must identify them, by no means immediately follows from the falsity

of the pure structuralist thesis, as maintained by him. It remains open that they are specific objects, to be identified with ones characterisable in some different way; or that they are specific objects, but characterisable only *as* numbers; or that, as Benacerraf believes, that they are not specific objects at all, even though capable of being characterised by reference to their application rather than by pure structure. The question touches upon a critical issue, nevertheless: one, in fact, that Frege came to recognise as the most critical for his entire philosophy of arithmetic. If numbers are logical objects, and yet capable of being *defined* as specific objects, the first of the above three possibilities must be correct. In this case, the definition must represent them, not simply as numbers, but as particular members of some more general range of logical objects, classes or value-ranges. The process of definition must stop somewhere, however. When it stops, how can the objects at which it stops be identified other than as objects of whatever kind they are? And how can this amount to anything but identifying them structurally, that is, by their particular roles within that range of logical objects? Must we not eventually come upon a fundamental realm of mathematical objects the only account of which will consist of a description, in the spirit of Dedekind, of its internal structure? Even if Dedekind's account of number theory is to be rejected, may we not be forced to offer a similar account of whatever lies at those foundations of arithmetic with which Frege was concerned? This was the challenge with which Frege ought to have seen Dedekind's work as presenting him; we shall see in due course how close he came to meeting it.

CHAPTER 6

Numerical Equations and Arithmetical Laws

The status of numerical equations

From § 5 to § 44 of *Grundlagen*, Frege occupies himself with a far-ranging critique of the answers proposed by a wide cluster of philosophers and mathematicians to the questions he has raised concerning arithmetic. This critique is intended to make Frege's own views, by the time he comes to present them, appear not merely plausible but inescapable, all alternatives having been demonstrated to be untenable. It is arranged with great skill, not merely to accomplish this, but to establish in succession a number of positive points.

(a) Kant

§§ 5 to 17 are devoted to the status of arithmetical propositions. Frege begins by drawing the obvious distinction between numerical equations and general laws, and gives his attention to the former, considering only those that involve addition. In a few devastating sentences he ridicules and utterly refutes Kant's account of these as synthetic but unprovable, though not classifiable as axioms because of the infinite number of them and their lack of generality. He begins by observing that, when the numbers involved are sufficiently large, as in ' $135664 + 37863 = 173527$ ', such equations are not self-evident. He thus sounds for the first time a note of frequent occurrence in the critical sections of *Grundlagen*: this or that theory fails for large numbers, or for the number 1, or for the number 0. Kant, Frege says, uses the lack of self-evidence on the part of such equations as an argument for their being synthetic. But, he comments, it rather goes to show that they cannot be unprovable: for how, if they were, could we recognise them as true? Frege is here alluding to Kant's remark:¹

¹ *Kritik der reinen Vernunft*, B 16.

The arithmetical proposition is therefore always synthetic; and this may be perceived the more clearly when one takes somewhat larger numbers . . .

Referring to Kant's immediately preceding observation, which relates to his favourite equation ' $7 + 5 = 12$ ':²

One has to go beyond these concepts [of seven and five] by calling in aid an intuition corresponding to one of them, say of one's five fingers, or . . . of five points, . . .

Frege comments, 'Kant wishes to call in aid an intuition of fingers or points. He thus runs the risk of making these propositions appear empirical, contrary to his own opinion; for an intuition of 37863 fingers is in any case not a pure one.' On Kant's own theories, only a pure intuition could underlie a synthetic a priori truth. The upshot is that, if numerical equations are a priori, they must be provable.

(b) Leibniz

Frege then turns, in § 6, to Leibniz, who gives a purported proof of ' $2 + 2 = 4$ ' from definitions of each number from 2 to 4 as the result of adding 1 to its predecessor. He observes that Leibniz's proof tacitly assumes the associative law for addition: 'if this law is assumed, it may be easily seen that we can in this way prove' every numerical equation in addition. The observation that there is a gap in Leibniz's proof is not a mere passing cavil at an oversight on his part: it is of critical importance for the ensuing discussion, as establishing that even numerical equations cannot be proved without appeal to some general arithmetical law, and enables Frege to fasten attention on the character of such laws. He pauses to commend Leibniz's idea of defining each individual positive integer from 1 and the operation of adding 1, remarking that he sees no other way in which they could be defined; here, then, is another positive result established at an early stage.

(c) Grassmann

To give such proofs of numerical equations in addition, we need the associative law only in the special form

$$a + (b + 1) = (a + b) + 1.$$

Frege observes that Grassmann tries to obtain the law in this form by definition.

² B 15.

As Frege comments, such a definition must be of the operation of addition:³ Grassmann in effect defines it as that operation for which the above general equation holds. It needs to be remarked, although it is not by Frege, that such a definition presupposes the meaning of ' $a + 1$ ' as already known: if a successor operation were explicitly invoked, the definition could take the form that '+' was stipulated to be that binary operation that satisfies the two equations:

$$\begin{aligned} a + 1 &= a' \\ a + b' &= (a + b)' \end{aligned}$$

where ' a ' denotes the successor of a . These then form the recursion equations for addition over the positive integers, just as they were subsequently given by Dedekind. Frege's criticism is that ' $a + b$ ' would be an empty symbol, if there was no operation, or more than one, satisfying these conditions: 'Grassmann simply assumes without proof that this does not happen, so that the rigour is only apparent.'

The definition would be justified by the theorem proved by Dedekind, establishing the existence of a unique function specified by primitive recursion, that is to say, by an arbitrary pair of recursion equations. It is precisely such a justification which Frege is demanding. It would furnish an example of those 'propositions on which the admissibility of a definition rests' to which he referred in § 3. In his middle period, Frege developed a very rigorist view of definitions, barring any that required some proposition to be proved before it could be admitted; quite evidently, when he wrote *Grundlagen*, he had as yet no objection to such definitions, provided that the necessary justification was supplied. Whether or not he here had in mind the specific justification that Grassmann's definition required, it is impossible to say. If he did, he was very close to Dedekind's demonstration of the validity of recursive definitions; but, even if so, he eventually preferred a definition connecting addition more directly with cardinality.

Thus, at the end of § 6, we have reached the conclusion that numerical equations in addition cannot be proved from definitions of the individual positive integers alone. Granted that all those greater than 1 are to be defined in terms of 1 and the successor operation, such equations can be proved with the help of a general law, namely a special case of the associative law for addition, or, otherwise expressed, the second recursion equation. This law may possibly be derivable from a suitable definition of addition; but, if so, that definition will itself need to be justified by means of a general theorem, which Frege does not state explicitly, but is in fact the theorem validating recursive

³ Frege writes, slightly obscurely, that the objection that this definition of the sum is circular 'can perhaps be evaded if we say . . . that what he is intending to define is not sum but addition'. The remark is accurate: the recursion equations do not of themselves constitute a definition of '+', i.e. do not allow it to be eliminated from all contexts; but we may legitimately define addition to be the unique binary function satisfying those equations – though the definition requires justification. (Otherwise expressed, '+' is second-order, but not first-order, definable from 1 and successor.)

definitions. The status of numerical equations, as analytic or synthetic, a posteriori or a priori, thus for the time being remains undecided: as Frege observes at the beginning of § 7, it will depend on that of whichever general law is appealed to in proving them.

(d) *Mill*

Or so one would think; but, as he goes on to remark, Mill denies this dependency. Although one might have expected Frege at this point to enquire more closely into the justification of the associative law, the mention of Mill diverts us along a different path; §§ 7–11 are now devoted to a critique of Mill's empiricist philosophy of arithmetic. Mill appears to accept that each particular number after 1 must be defined as the result of adding 1 to its predecessor; for he says that we may call 'Three is two and one' a definition of the number three,⁴ and later that 'Each number is considered as formed by the addition of a unit to the number next below it in magnitude'.⁵ He holds, however, that these 'are definitions in the geometrical sense, not the logical; asserting not the meaning of a term only, but along with it an observed matter of fact'.⁶ The observed fact corresponding to the definition of '3' is claimed to be that 'collections of objects exist, which while they impress the senses thus, ° ° °, may be separated into two parts, thus, ° ° °'. 'What a mercy, then', comments Frege, 'that not everything is nailed fast' (§ 7). Having enquired after the physical facts underlying the numbers 0 and 1, unfairly so because Mill does not suppose them to be defined in this way, Frege invokes the universal applicability of number: on Mill's account, it would be incorrect to speak of three strokes of the clock, three tastes or three solutions of an equation. (Strictly speaking, Frege is wrong to cite the clock, since, in asking how often it struck, we are calling for an ordinal, not a cardinal, number.)

Mill gives a proof of ' $5 + 2 = 7$ ' after the same fashion as Leibniz's proof of ' $2 + 2 = 4$ ', making a similar surreptitious appeal to associativity;⁷ but he asserts that such equations 'do not follow from the definition itself, but from' the observed matter of fact.⁸ This piece of carelessness, characteristic of the great empiricist, enables Frege to ask where in the proof the observed fact should have been cited; if Mill had allowed that such equations did follow from the definitions, but had claimed for them an empirical status on the ground that the definitions themselves rested on empirical facts, his position would have been stronger. Even so, Frege urges that he would have no escape from having to maintain that we observe facts relating to every individual

⁴ *System of Logic*, book II, chap. VI, § 2.

⁵ *Ibid.*, book III, chap. XXIV, § 5.

⁶ Book II, chap. VI, § 2.

⁷ Book III, chap. XXIV, § 5.

⁸ Book II, chap. VI, § 2.

number that we mention; for no suitable general principle covering all of them, and obtained by empirical induction, can be framed. Variations on these arguments are pressed in § 8: the upshot is that the definitions of individual numbers, in terms of 0 or 1 and the successor operation, 'neither assert observed facts nor presuppose them for their justifiability' (§ 9).

Application

Since the numerical equations are derivable from those definitions with the help of some general law, Frege proceeds in § 9 to ask after the nature of such laws. Can they be highly general laws of nature, arrived at by empirical induction, as Mill maintains? Here we come upon a feature of *Grundlagen* which inevitably causes any reader who has not also read Part III of *Grundgesetze* to form an incorrect picture of Frege's philosophy of mathematics. For Mill, the inductive truths governing the operation of addition are arrived at by observing the results of physically adjoining two aggregates of physical objects. 'What the name of number connotes is the manner in which single objects of the given kind must be put together, in order to produce that particular aggregate.' To form 'the aggregate which we call *four*', for example, 'two aggregates of the kind called *two* may be united; or one pebble may be added to an aggregate of the kind called *three*';⁹ in this way arithmetical laws are 'in reality physical truths obtained by observation'.¹⁰ This leads Frege to make the wholly justified accusation:

Mill always confuses the applications that can be made of an arithmetical proposition, which are often physical and do presuppose observed facts, with the pure mathematical proposition itself. The plus sign may indeed seem, in many applications, to correspond to a process of aggregation. But that is not its meaning: for in other applications there is no question of heaps or of aggregates, or of the relationship of a physical body to its parts, for example when the calculation relates to events.

As a criticism of Mill, one reiterated in the footnote to § 17, this is completely apt. The point is repeated in § 16, where Frege says of applications of arithmetic to physics:

It is . . . a mistake to see in such applications the real sense of the propositions; in any application a large part of their generality is always lost, and something particular enters in, which, in other applications, is replaced by something else.

These remarks naturally induce the reader of *Grundlagen* to interpret Frege as an advocate of the inviolable purity of mathematics, for whom its external

⁹ *System of Logic*, book III, chap. XXIV, § 5.

¹⁰ *Ibid.*, § 7.

applications are adventitious and irrelevant to their essence. It comes as an enormous surprise to such a reader, therefore, to come in *Grundgesetze*¹¹ upon the statement that

It is applicability alone that raises arithmetic from the rank of a game to that of a science. Applicability therefore belongs to it of necessity.

This might be thought to represent a complete change of view; but a little later in *Grundgesetze* we find Frege criticising Helmholtz in terms very similar to those in which he had criticised Mill in *Grundlagen* as one of those who 'confuse the applications of arithmetical propositions with the propositions themselves'.¹² He continues:

As if the questions as to the truth of a proposition and as to its applicability were not quite distinct! I can very well recognise the truth of a proposition, without knowing whether any application can be made of it.

Helmholtz, like Mill, was a proponent of an empiricist philosophy of mathematics. It is when he is criticising empiricism that Frege insists on the gulf between the senses of mathematical propositions and their applications; it is when he criticises formalism that he stresses that applicability is essential to mathematics. Formalism, properly so called, is not considered in *Grundlagen*, only its more timid cousin, postulationism: and therefore Frege has no occasion in that book to sound the latter of these two notes.

At first sight, there is a flagrant contradiction between what Frege says in the one connection and what he says in the other; but the appearance is illusory. Any specific type of application will involve empirical, or at least non-logical, concepts alien to arithmetic; very often, it will depend upon empirical presuppositions. To make such applications intrinsic to the sense of arithmetical propositions is therefore to import into their content something foreign to it, and to render their truth synthetic: that is the mistake of Mill and Helmholtz. What *is* intrinsic to their sense, however, is the general principle governing all possible applications. That must accordingly be incorporated into the definitions of the fundamental arithmetical notions. It is not enough that they be defined in such a way that the possibility of these applications is subsequently provable; since their capacity to be applied in these ways is of their essence, the definitions must be so framed as to display that capacity explicitly.

In *Grundlagen*, Frege did not expound this aspect of his philosophy of arithmetic (in the sense in which 'arithmetic' embraces analysis as well as number theory), and therefore it is not apparent to a reader of that book. He set it forth only in Part III of *Grundgesetze*, which is devoted to his theory of

¹¹ Vol. II, § 91.

¹² Vol. II, § 137, fn. 2.

real numbers and is among the least read of his writings. It is there apparent, not only from his explicit statements, but from his rejection of rival means of constructing the real numbers, such as those of Dedekind and Cantor, and from the method of constructing them which he himself adopts. It comes out clearly from his criticism of Cantor, in particular, that his fundamental ground for rejecting rival theories like Cantor's was their failure to satisfy the demand that the principle governing all possible applications of the real numbers should be displayed by their definition. *Any* system of objects having the mathematical structure of the continuum is *capable* of the same applications as the real numbers; but, for Frege, only those objects directly defined as being so applicable could be recognised as being the real numbers. It is not only Frege's theory of real numbers that is overlooked by one who neglects to study Volume II of *Grundgesetze*, but a leading component of his general philosophy of mathematics.

This component is present in *Grundlagen*, too, but far from obviously. The rival theories of natural numbers reviewed in that book do not, of course, include that of Dedekind, which wanted another four years until publication. But Frege's deepest objection to it would have been that it attempted to characterise the totality of natural numbers purely in terms of its internal structure, and relegated their application as finite cardinals to an appendix to the theory. For Frege, conversely, that was the salient type of application that could be made of the natural numbers, and hence must be made, as he made it, central to their definition. It was sufficiently general for such a purpose, being quite unspecific as to the type of objects concerning which the question 'How many?' could be answered by citing a natural number; for that reason, it involved no concept peculiar to any non-mathematical subject-matter. Without the discussion of real numbers in *Grundgesetze*, Part III, it would be easy to suppose that Frege's definition of the natural numbers as finite cardinals in *Grundlagen* was due only to a certain traditionalism in his approach, or at most to his desire to characterise them, without appeal to the psychological process of abstraction, as quite specific objects. Doubtless both motives operated. Perhaps, too, the possibility of a purely structural characterisation had not so much as occurred to him. But the procedure which he adopted in his construction of the natural numbers was in complete consonance with what was later to appear as one of the principal strands of his philosophy of mathematics. Far from insisting on the purity of mathematics, and treating its applications as philosophically irrelevant, he is, among all the philosophers of mathematics, that one who assigned to applicability its most central place.

General arithmetical laws

If the general laws of arithmetic were based on induction in the scientific sense, Frege argues in § 10 of *Grundlagen*, they would have to be arrived at

from numerical equations (for example, by inferring the general validity of the commutative law for addition from the truth of a large number of its instances). Since the equations can be proved from the definitions of the individual numbers only with the help of some general law, we should thus lose the whole advantage of those definitions, and should have to find some other way of establishing the numerical equations. Besides, the natural numbers are not all alike, as are points in space or moments in time. Each has its particular properties: we cannot say that what happens at one place in the sequence of natural numbers must happen at any other, as we say that what happens at any spatio-temporal position must happen at any other, if the conditions are the same.

These considerations are really pointless: in § 10, Frege has, for a moment, lost the thread of his argument. Once he has, by distinguishing the sense of an arithmetical proposition from its physical applications, refuted Mill's idea that arithmetical truths are attained by induction from the results of physical operations, nothing more needs to be said about their supposedly inductive character. It is wrong to argue, as Frege in effect does, that we cannot arrive at number-theoretic conjectures on this basis: Goldbach's conjecture is an obvious counter-example, and there are many more. It is equally obvious that we do not trust such conjectures, which prove to be mistaken as frequently as they prove correct, and that we certainly do not assert them as true before we hit on a proof, or at least have, in the shape of a computer proof, empirical evidence that there is a proof; there is no a priori reason why the smallest counter-example to a generalisation should not be very large.

Before bringing § 10 to a close, Frege introduces an image, that of the borehole, which has no probative force, but suggests an important principle. He supposes that the drill has so far penetrated a sequence of very different rock strata, but that we have noticed that the temperature increases uniformly with the depth; and he comments, somewhat unreasonably, that we cannot presume that the temperature would continue to increase at the same rate with further drilling. We can indeed form the concept 'what will be encountered by continued drilling', but can deduce only what is determined solely by the depth of a stratum, and that without needing to invoke empirical induction. Similarly, we can form the concept 'whatever is obtained by repeatedly increasing by 1'. The difference is that we simply *encounter* the strata reached by going on with the drilling, whereas the numbers are *constructed* by the repeated addition of 1, and hence their very natures are thereby determined; it follows that all their properties can be deduced from the specific way each was so generated. That, however, amounts to saying that the properties of each number follow from its definition. It furthermore 'opens up the possibility of proving the general laws of numbers from the method of generation common to all of them'. This is a way of saying that the general laws of arithmetic are to be proved by mathematical induction, as the associative law can be proved from the recursion

equations for addition; and it hints at the method Frege will adopt for defining the concept *natural number*, namely as comprising all and only those objects attainable from 0 by reiterating the successor operation.

Intuition

In § 12, assuming his reader's agreement that he has refuted the view that the general laws of arithmetic are a posteriori truths, Frege asks whether they are synthetic a priori or analytic. Kant, as he remarks, held them to be synthetic a priori; and, in this case, Frege comments, we have no alternative but to invoke a pure intuition as the ultimate ground of our knowledge of them. Despite his previous sarcasm about the intuition of 37863 fingers, and despite his observation in this section that 'we appeal too readily to inner intuition when we cannot cite any other ground of knowledge', Frege should not be understood as disparaging the whole notion of pure intuitions. In *Grundlagen*, § 89, he says expressly that, 'in calling geometrical truths synthetic and a priori, [Kant] revealed their true nature'; we must conclude, from the comment in § 12, that he regarded our knowledge of them as resting on pure intuition. Mentioning certain contemporaries who agreed with Kant about arithmetical truths, Frege cites from Hankel's book on analysis the phrase 'the pure intuition of magnitude'.¹³ He comments:

If we consider everything that is called a magnitude: numbers (*Anzahlen*), lengths, areas, volumes, angles, curvatures, masses, velocities, forces, intensities of illumination, electric currents, etc., it is easy to understand how they can all be brought under one *concept* of magnitude; but the expression 'intuition of magnitude', let alone 'pure intuition of magnitude', cannot be recognised as correct.

Ten years previously, at the beginning of his Habilitationsschrift (post-doctoral dissertation) of 1874,¹⁴ Frege had expressed the same view, arguing that the concept of magnitude had been gradually disentangled from intuition and that its connection with intuition had in any case been illusory: lines and plane figures are intuitable, 'but precisely what constitutes their magnitude, what lengths and areas have in common, eludes intuition'. The concept of magnitude has far too great generality to be derivable from intuition.

In the 'Rechnungsmethoden' Frege discussed the concept of magnitude in detail, and in Part III of *Grundgesetze* he attempted a precise mathematical analysis of it. Here, however, it is really an irrelevancy, which Frege attempted to disguise by listing *Anzahlen* (cardinal numbers) in the above quotation as forming a species of the genus magnitude. Properly speaking, however, they do not; but Frege seems to have been far less clear about this in *Grundlagen*

¹³ *Theorie der complexen Zahlssysteme*, Leipzig, 1867, pp. 54–5.

¹⁴ 'Rechnungsmethoden, die sich auf eine Erweiterung des Grössenbegriffes gründen' ('Methods of Calculation based on an Extension of the Concept of Magnitude').

than he later was in *Grundgesetze*. As he repeatedly remarks, a natural number serves to answer a question of the form ‘How many?’: more precisely, ‘How many objects are there which satisfy such-and-such a condition?’. ‘How many miles to Babylon?’ is not of this form; Frege is wrong, in *Grundlagen*, § 19, to assert that it is, saying that ‘a number that gives the answer to the question, “How many?”’, can also determine how many units are contained in a given length’. The magnitude of a quantity, on the other hand, may be specified by citing a rational or real number, together with a unit of measurement; to ask after it is to pose a question of the quite different form ‘How much?’ or ‘How great?’. If Frege did not draw the distinction sufficiently sharply in *Grundlagen*, he did so in *Grundgesetze*, where he says, ‘Anzahlen [cardinal numbers] answer the question, “How many objects of a certain kind are there?”’, while the real numbers may be considered as numbers used for measurement, which state how great a quantity is compared with a unit quantity’.¹⁵

The crucial question, therefore, is whether we can speak of intuitions of the natural numbers. Frege declares outright that we have no intuition of a large number such as 100,000, and proceeds to consider Kant’s notion of intuition. He quotes from Kant’s *Logik* (§ 1) his distinction, among ideas, between intuitions and concepts:

An intuition is an *individual* idea [*Vorstellung*] (REPRESENTATIO SINGULARIS), a concept a *general* idea (REPRESENTATIO PER NOTAS COMMUNES) or *reflective* idea (REPRESENTATIO DISCURSIVA).

He comments that:

Here there is absolutely no mention of any relation to sensibility, which, on the other hand, is associated with intuition in the Transcendental Aesthetic, and without which intuition cannot serve as the principle of our knowledge of synthetic judgements a priori.

Citing Kant’s statement that ‘it is therefore through the medium of sensibility that objects are *given* to us, and it alone furnishes us with intuitions’,¹⁶ Frege concludes that he used the term ‘intuition’ in a wider sense in the *Logik* than in the *Kritik*. Frege allows that, in the former sense, 100,000 might be called an intuition, since it is certainly not a general concept; but, in this sense, an intuition cannot serve as the foundation of arithmetical laws.

Frege’s complaint is misstated. Kant had no intention in the *Kritik* of making dependence on sensibility part of the *definition* of the word ‘*Anschauung*’ (conventionally translated ‘intuition’ when used by Kant or Frege), or of using it in the *Kritik* in any narrower sense than that given by the definition in the *Logik*; if he had, it would have been the merest triviality to say that sensibility

¹⁵ Vol. II, § 157.

¹⁶ *Kritik der reinen Vernunft*, B 33.

alone can furnish us with intuitions. On the contrary, he gave a precisely parallel explanation. Having explained that a perception (*Perception*) is an idea of that particular kind which involves awareness (*Vorstellung mit Bewusstsein*), he says that such a perception may be either a sensation (*Empfindung*) or a cognition (*Erkenntnis*). The latter are subdivided into intuitions and concepts, the difference between which he explains as follows:¹⁷

An objective perception is a *cognition*. This is either an *intuition* or a *concept* . . . The former relates directly to an object and is individual; the latter relates to it indirectly by means of a characteristic (*Merkmal*) which can be common to several things.

It was, rather, a thesis maintained by Kant that all our intuitions – our ideas of individual objects – are sensible in character. This thesis Frege rejected, as he says expressly in § 89 of *Grundlagen*:¹⁸ numbers, for him, are objects, and it is evident at least for very large numbers that they are not given to us intuitively, if this involves perception or sensory imagination. For this refutation of Kant's thesis to be effective, however, it must have been established that numbers *are* objects. In 'Booles rechnende Logik', Frege had remarked that 'the number 3 is not to be regarded as a concept, since the question what can fall under it is senseless': no object can be three in number. He had no right, however, save as an *argumentum ad hominem*, to invoke Kant's dichotomy between objects and concepts, since he admits other logical types: relations (§ 70) and concepts of second order or level (§ 53). In a sense, he preserves the dichotomy, since he regards both relations and second-level concepts as concepts of a kind; but the fact that we cannot speak of an object's falling under the concept 3 has no tendency, on Frege's own principles, to show that the number 3 is an object.

This, then, is one refinement that Frege makes of Kant's classification: the class of concepts is to be subdivided, so as to admit, besides first-level concepts proper, relation-concepts and concepts of second level (called in *Grundlagen* 'of second order'). A second emendation is the distinction between objective and subjective ideas, which in the footnote to § 27 Frege condemns Kant for failing to draw. Unknown to Frege, precisely the same demand for a sharp distinction between the subjective and objective senses of the word '*Vorstellung*' had previously been made by Bolzano in his *Wissenschaftslehre* of 1837. Bolzano understood it as covering, in its subjective sense, sensations, mental images and the like, and, in its objective sense, constituents of what he called 'propositions in themselves', which correspond to what, in his middle period, Frege called 'thoughts'. He even laid down the same principle of differentiation as

¹⁷ *Kritik*, B 376–7.

¹⁸ Quoting, this time, from the *Kritik*, B 75.

that on which Frege was to insist, namely that subjective ideas require a subject or bearer, whereas objective ones do not.¹⁹

From § 27 onwards, Frege's terminology diverges markedly from Kant's. Kant's term '*Vorstellung*' (standardly translated 'idea' as used by Frege, but 'representation' as used by Kant, although Kant intended it as the equivalent of 'idea' and '*idée*' as used in philosophical writing in English and French) is henceforth reserved by Frege for subjective ideas, that is, elements of the stream of consciousness such as mental images. Unlike Bolzano, who retained the Kantian distinction between intuitions and concepts within the realm of objective ideas, as well as within that of subjective ones, Frege chose to treat the term 'intuition' as applying only to occupants of the subjective side of the classification; 'concept', on the other hand, was used by him exclusively for certain kinds of objective idea. Thus, in Frege's revised terminology, the opposition between intuitions and concepts is misconceived: what is correlative to the notion of a concept is that of an object.

Frege, in his early period, was alive to the difference between sign and thing signified; given the distinction between subjective and objective ideas, he as yet saw no need for any further differentiation between an objective idea and that of which it is the idea. This latter distinction is that between sense and reference, which, when he later came to draw it, was a distinction *within* the realm of the objective. In § 12 he has not yet introduced the subjective/objective distinction; this is what makes it possible for him to say, not that we may perhaps have an intuition of 100,000 in the sense of 'intuition' explained in Kant's *Logik*, but that 100,000 may perhaps *be* an intuition in that sense. It does not disturb us to miss, in *Grundlagen*, any differentiation between the idea and that of which it is the idea when he speaks of concepts; but the lack of any such differentiation for objects appears to us deeply shocking, and it is with dismay that we read, in the footnote to § 27, that 'objective ideas can be divided into objects and concepts'. In *Grundlagen*, however, Frege considered the distinction between objective and subjective ideas to be sufficient; when, later, he made the distinction between objective ideas and that of which they are ideas, he made it uniformly for objects and concepts. 'Objective ideas' were transformed into senses; objects and concepts were what such objective ideas were ideas of.

The brilliance and clarity of *Grundlagen*, and the cogency of many of its arguments, make it difficult for us to take in the fact of Frege's blindness, during the whole of his early period, to what seems to us an obvious need for a distinction. He simply had no consciousness, until he formulated the principles of his middle-period theory, of the necessity for distinguishing between the significance of an expression and that which it signifies. The

¹⁹ *Wissenschaftslehre*, §§ 19, 48 and 270–3.

switch from speaking of numbers as intuitions in the sense of Kant's *Logik* to speaking of them as objects was not intended to mark the difference between ideas and what they are ideas of, but that between ideas of the subjective variety and those of the objective variety. The content or meaning (*Bedeutung*) of an expression was for Frege at that time simultaneously its significance and *what* it signified: the distinction became apparent to him only when he drew his distinction between *Sinn* and *Bedeutung*, and he was strictly accurate in saying that he had split the former notion of content into those two components.²⁰ This explains the oddity of his later terminology: he chose to retain the term '*Bedeutung*' for that which the expression signifies. It explains also why the term 'concept' plays so striking a double role in *Grundlagen*, being used sometimes for the sense of a predicative expression and sometimes for its reference. Naturally, no coherent exposition can be given of the doctrines of *Grundlagen* without acknowledging a distinction between significance and what is signified; but, in reading the book, we must bear in mind the fact that Frege was not himself making such a distinction. His failure to do so means that there was at that time a radical incoherence at the very heart of his thinking, though one that obtrudes very little in the argumentation of the book. It is for this reason that it is so misguided to try, as writers like Baker, Hacker and Shanker have done, to read into the work of Frege's early period a system of philosophical logic different from the theory presented in his middle period, but equally worked out and articulated.

The conclusive proof that numbers are not intuitable had in fact been given before § 12. In § 5 Frege had said that, if we had an intuition of 135664 fingers, another of 37863 fingers and a third of 173527 fingers, the correctness of the equation ' $135664 + 37863 = 173527$ ' 'would have to be immediately evident, at least for fingers': for Frege, intuitions involve not only particularity, but immediacy. It could easily be maintained, however, that arithmetic needs to appeal to intuition, without believing that we have intuitions of the individual numbers; the only certain way to refute the claim is by framing definitions, and supplying proofs, that show the recourse to intuition otiose. Bolzano had begun the process of eliminating intuition from analysis by proving something apparently obvious to geometrical intuition, namely the mean value theorem, stating that a continuous function on the reals must assume the value 0 at some point in an interval in which it has both positive and negative values.²¹ Bolzano expressly proclaimed the value of proving apparently obvious statements, in order to establish on what they actually rest, and, in particular, that they do not depend on spatial intuition; but, as he showed in his example of

²⁰ *Grundgesetze*, vol. I, Preface, p. x.

²¹ *Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwei Werten, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liegt*, Prague, 1817.

a continuous but nowhere differentiable function, what appears obvious may not even be true.²²

One of Frege's aims was to accomplish the same for number theory. Already in 'Rechnungsmethoden' he had pronounced on the difference in this respect between arithmetic and geometry:

There is a remarkable difference between geometry and arithmetic in the way in which their fundamental principles are based. The elements of all geometrical constructions are intuitions, and geometry appeals to intuition as the source of its axioms. Since the subject-matter of arithmetic is not intuitable, its fundamental principles cannot likewise spring from intuition.

In the Preface to *Begriffsschrift*, having distinguished analytic and synthetic propositions, he declared his ambition to demonstrate arithmetical laws to be of the former kind:

Having posed to myself the question to which of these two kinds arithmetical judgements belonged, I had first to see how far one could get in arithmetic by means only of inferences based purely on the laws of thought, which rise above everything particular. The path I followed was first to try to reduce the concept of ordering in a sequence to *logical* succession, in order to advance from there to the concept of number. So that nothing from intuition should intrude, everything had to depend upon the absence of any gaps in the chain of inference.

Accustomed as we are to the geometrical representation of a function on the real numbers, we find it unsurprising that fundamental theorems of analysis should once have been supposed to rest upon spatial intuition, so that it needed the efforts of Bolzano and his successors such as Cauchy and Weierstrass to expel intuition from the theory. It is less obvious that this should have needed to be done for number theory; but Frege rightly fastened on the concept of a sequence, which it was natural to discuss in terms of a temporal process of moving from term to term. In *Begriffsschrift*, Frege succeeded in giving a purely logical analysis of the concept by means of his celebrated definition of the ancestral of a relation; and at the beginning of Part III of that work, he emphasised its philosophical significance:

One sees from this example how pure thought alone, prescindng from any content given by the senses or even by an a priori intuition, is capable of eliciting, from a content that arises out of its own constitution, judgements that at first sight seem to be possible only on the basis of some intuition . . . The propositions about sequences developed in the following far surpass in generality all similar ones that can be derived from any intuition of sequences. If anyone were to regard it as more appropriate to use an intuitive idea of sequence as a basis, he should

²² The example was given in his *Funktionenlehre*, written in about 1830, but not published until a century later, in *Bernhard Bolzanos Schriften*, vol. I, Prague, 1930, having been first reported by M. Jasek in 1921.

not forget that the propositions thus obtained, which might coincide verbally with those given here, would yet assert far less than do the latter, because they would hold good only in the domain of that intuition on which they were based.

A concept of sequence based on temporal intuition could be applied only to temporal sequences, one based on spatial intuition only to spatial ones; but the concept of a sequence has a far greater generality, since the relation between one term and the next may be of any kind whatever, and this is what makes it definable by purely logical means.

In § 91 of *Grundlagen*, Frege cites the very last theorem of *Begriffsschrift* as an example of a proposition that might at first sight be taken to be synthetic, but which he has been able to prove 'without borrowing any axiom from intuition'; applied to natural numbers, the theorem yields the law of trichotomy. Likewise, in § 80, he uses the ancestral to define the sequence of natural numbers, and remarks that it is only by means of this definition that it is possible to reduce mathematical induction, an inference apparently peculiar to mathematics, to the general laws of logic. He comments on the definition of the ancestral:

Since the relation ϕ has been left indefinite, the sequence is not necessarily to be conceived in the form of a spatial and temporal ordering, although these cases are not excluded. Some might perhaps regard another definition as more natural, for instance: if, starting from x , we always transfer our attention from one object to another to which it stands in the relation ϕ , and if in this way we can finally reach y , we say that y follows x in the ϕ -sequence. This is a way of investigating the matter, not a definition. Whether we reach y by transferring our attention may depend on a variety of subjective surrounding circumstances . . . Whether y follows x in the ϕ -sequence has in general nothing to do with our attention and the conditions for transferring it.

He concludes that 'by means of my definition the matter is raised from the domain of subjective possibilities to that of the objectively determinate'.

In § 12, these achievements are as yet unknown to the reader; but Frege has the resources to incline him towards the belief that he wishes to establish, that arithmetical truths are analytic. § 13 is devoted to a preliminary contrast between arithmetic and geometry: 'if in geometry general propositions are derived from intuition, that is explicable from the fact that the points, lines and planes intuited are not really particular ones, and so can serve as representatives of their entire kind.' Nothing similar is possible in arithmetic, because, as previously remarked, no one number is entirely like another. § 14 is the magnificent piece of rhetoric quoted in a previous chapter, in which Frege declares that, although we cannot imagine non-Euclidean space, it can be encompassed by conceptual thought, so that the laws of (Euclidean) geometry do not govern everything thinkable, whereas everything falls into confusion if we attempt to deny any of the laws of arithmetic. Our knowledge of these laws

cannot, therefore, be based on intuition: the basis of arithmetic lies deeper even than that of geometry. § 15 cites Leibniz and W.S. Jevons as favouring the analyticity of arithmetical truth. § 16 raises against this the difficulty posed by the question, 'How do the empty forms of logic come to disgorge so rich a content?' (in Austin's fine, though free, translation). This is the problem of the value of analytic judgements, to which Frege is not yet ready to expound his solution: he contents himself, in § 17, with pointing out that it coincides with that of the fruitfulness of deductive reasoning, since we can always transform any valid piece of reasoning into an analytic truth by framing the conditional whose antecedent is the conjunction of the premisses and whose consequent is the conclusion. Such a transformation has the advantage that it 'leads to a general proposition, which need not be applicable only to the facts immediately under consideration'. Frege, as a philosopher of mathematics placing the greatest emphasis on its applications, was highly conscious that the ultimate point of establishing analytic truths is to enable us rapidly to infer non-logical conclusions from non-logical premisses.

The complaint is sometimes made that Frege concentrated on the finished mathematical product, not on the process of constructing it. Of this he was perfectly well aware: the questions he was concerned to ask concerned the finished product; he assumed only that no considerations about the process of arriving at it would invalidate his answers to those questions. § 17 contains a quotation from Leibniz which exactly states Frege's point of view: 'it is here a matter, not of the history of our discoveries, which is different in different people, but of the connection and natural order of truths, which is always the same.'

In the very first sentence of the Introduction to *Grundgesetze* Frege stated, more accurately than he had done in *Grundlagen* itself, what he took the aim of *Grundlagen* to be:

In my *Grundlagen der Arithmetik* I sought to make it probable that arithmetic is a branch of logic and needs to borrow no ground of proof either from experience or from intuition.

That was indeed Frege's central concern: not to arrive at certainty concerning the truths of arithmetic, but to establish the *ground* for our acceptance of them, and, in particular, to refute the belief that intuition was among those grounds; and, in this regard, he was following in the footsteps of Bolzano. Formalisation of proofs would unquestionably increase certainty, when this was in any degree in doubt; but that was not Frege's objective in formalising them. His aim was, rather, to achieve certainty, not about the truth of arithmetical theorems, but about what was needed to establish their truth. In § 90 of *Grundlagen*, Frege conceded that he had not, in that book, conclusively demonstrated the derivability of the truths of arithmetic from the laws of logic alone: that could be

done only when their proofs were fully formalised. If we are content with the unformalised proofs usual in mathematics, either of two opposite errors may occur. Some seemingly self-evident step in the proof may in fact depend in part on intuition, rather than representing a purely logical inference; or, conversely, some purely logical transition may be taken to rest upon intuition, because it fails to conform to any recognised form of logical inference, when in fact it could be broken down into a sequence of shorter but purely logical steps. Hence *Grundlagen* itself could do no more than make it probable that arithmetical truths depended upon the laws of logic alone: to establish that, it was necessary to present fully formalised proofs of them from fundamental logical principles; that, of course, was to be the task of *Grundgesetze*.

Part I of *Grundlagen* ends with § 17. At the beginning of § 18, Frege records once more the decision reached to define the individual numbers in terms of 1 and successor, and remarks that these definitions remain to be completed by defining the number 1 and the successor operation. Further, even to derive numerical equations, we need in addition some general laws, which, in virtue of their generality, can follow only from the general concept of number (*Anzahl*). It is to views concerning this concept that Part II is devoted.

CHAPTER 7

What is Number?

In *Grundlagen*, § 18, Frege reiterates that the individual numbers are to be defined in terms of 1 and successor; to complete the definitions, he says, we need to define 1 and the successor operation. Furthermore, he repeats, we need general laws even to derive numerical equations. 'Just because of their generality, such laws cannot follow from the definitions of the individual numbers', he declares, 'but only from the general concept of number'; the word he uses for 'number' in the phrase 'concept of number' is '*Anzahl*', i.e. 'cardinal number'. Frege was, indeed, right to think that, if we want to prove laws concerning general cardinal arithmetic, we have no choice but to define 'cardinal number'. We do not need laws of such generality in order to derive numerical equations concerning the positive integers, on the other hand; if we are concerned solely with the laws governing them, Dedekind's book disproves Frege's assertion. Provided that we know how to justify definition by recursion, and then define the arithmetical operations, addition, multiplication and any others we need, by that means, we can derive the arithmetical laws holding for the positive integers from the fact that they consist in whatever can be attained from 1 by repeated application of the successor operation, that is, from the principle of mathematical induction. Precisely this was implicit in Frege's earlier remarks, in § 10, about the borehole; here that insight seems to have slipped his mind.

That might seem to make little difference: for, if we defined the positive integers in that way, and supplemented this definition by specifying what object the number 1 is to be taken to be, and what operation is to be that of successor, we should thereby have fully explained the concept of positive integer. If Frege had taken this path, he would certainly so have defined 1 and successor as to elucidate the use of number-words to answer questions of the form 'How many . . . ?'; but it does not suit his purposes to take that path at this stage. Instead, he embarks on a review of answers to the question, 'What is number?', where, of course, this means primarily 'What does it mean to speak of the number of things of a given kind?'. The preliminary enquiry into answers to this question occupies Part II of *Grundlagen*, comprising §§ 18 to 28; its

completion takes up the greater part of Part III, from § 29 to § 44, which constitutes Frege's relentless critique of the abstractionist account of the matter.

The beginning is unpromising. In § 19, Frege cites Newton as proposing to define number as a ratio between quantities, which was to be his own characterisation of real numbers in *Grundgesetze*, Part III. Frege correctly remarks that Newton's explanation covers 'numbers in the wider sense, including fractions and irrationals'; but since, as we have already remarked, he does not appear in *Grundlagen* to distinguish these from cardinal numbers as sharply as he does in *Grundgesetze*, he flounders somewhat, and fails to make the simple point as cleanly as he ought.

After a brief dismissal, in § 20, of those who consider number indefinable, not because they know of any obstacle in principle to defining it, but only because attempts to define it have failed, Frege gets into his stride. The salient question of §§ 21 to 54 is: what is a number the number *of*? Alternatively expressed, it is: of *what* is a number a feature? A subsidiary question is what sort of feature a number is, and, in particular, whether it is an objective or a subjective feature. We may interpret the question 'What is a number the number of?' as follows. As Frege remarks in § 21, number-words occur in ordinary speech principally as adjectives. Tacitly, he assumes that any sentence containing a number-adjective can be transformed into what he calls an 'ascription of number' (*Zahlangabe*) (§ 47). An ascription of number is a direct answer to a question 'How many?', and takes the form 'There are (just) n . . .'. For instance, 'The Kaiser's coach is drawn by four horses' (§ 46) can be expressed as 'There are four horses that draw the Kaiser's coach'. The question then becomes 'What, in general, is an ascription of number *about*?', or, alternatively, 'What is a number ascribed to?'.

To us, it appears that there is a simple method of answering this question, easily applied. Suppose we had to answer the analogous question what, in general, a virtue is a feature of or is ascribed to. Expressions that we use to ascribe virtues are those like '. . . is honest', '. . . is generous', etc., in the sense of '. . . has an honest (generous, etc.) character'. The word or phrase that fills the gap in any such expression to complete the sentence denotes that to which the virtue is being ascribed. Hence, to determine to what, in general, a virtue is ascribed, we have to ask after the widest range of terms that can fill the gap in a virtue-predicate so as to yield an intelligible sentence. Obviously, an expression belongs to this range if and only if it is a term denoting a person; hence it is, in general, a person to whom a virtue is ascribed.

Of course, in that case the technique merely gives an answer that could have been given straight off; but this justifies the use of the technique in less obvious cases. In view of the generality of number – the fact that there is no restriction on the type of objects of which we can say how many there are – we can give no more specific answer to the question what, in general, can fill

the gap in an ascription of number than 'a predicative expression': an expression whose meaning consists in its applying, or not applying, to any given object. Frege explains, in § 74, that, as he uses the term 'concept', the general form of a judgeable content that is about an object is 'a falls under the concept *F*': otherwise expressed, whatever can be true of or false of an arbitrary object may be taken as standing for a concept. We thus arrive at the desired conclusion: that to which, in general, a number is ascribed is a concept.

This is the eventual answer to the question which Frege supplies in § 46, after rejecting in turn a sequence of different answers proposed by various philosophers and mathematicians. Indeed, to anyone who has been tempted by those answers, Frege's solution comes as a revelation, resolving all the difficulties they provoke and he has exposed. To us, however, the detour through these fallacious answers may seem a piece of unnecessary business. The correct answer could have been arrived at with no perplexity, simply by applying the technique just sketched. If it is not quite so obvious as the answer to 'To what is a virtue ascribed?', it seems nearly so: pages of discussion were surely not needed in order to light upon it.

Such a judgement would be quite superficial. The technique invoked for answering the question is essentially Frege's; and its validity depends upon acceptance of Frege's semantics. Until we have accepted that semantics, either in the rudimentary state it still assumed at the time when *Grundlagen* was written, or in the more developed state in which it appeared in the writings from 1891 onwards, we do not have the answer 'a concept' available as a possible one; even if the technique for arriving at an answer be employed, we are not in a position to say that the expression filling the gap in a sentence of the form 'There are $n \dots$ ' stands for a concept. The arguments Frege uses, in §§ 21–8, in favour of his answer to the question 'What is a number the number of?' and against answers proposed by others, are arguments for adopting his analysis of ascriptions of number. Since that analysis is both syntactic and semantic in character, they are also suasions in favour of his semantic theory: it is, among other reasons, *because* that theory is capable of giving a convincing account of ascriptions of number, and rival semantic theories are not, that we now take for granted the correctness of a semantics at least generally along Fregean lines, and do not so much as stop to consider one of those implicitly underlying the views Frege here so decisively refutes.

The discussion from § 22 to § 28 is in fact very economical. §§ 22–5 do much more than answer the question posed in § 21, whether number is a physical property; that is adequately dealt with by the appeal, in § 24, to the universal applicability of number: 'it would be remarkable if a property abstracted from external things could be transferred without change of sense to events, ideas and concepts . . . It is absurd that what is by nature sensible should occur in what is non-sensible.' If number were a physical property, we could not intelligibly talk of the number of figures of the syllogism. It is no

use to explain this in terms of the number of visual representations of the figures there may be: for we need to be satisfied that every figure is represented, and none represented more than once.

§ 22 accomplishes something different. First, it establishes the obvious fact that the plural subject of a sentence ending ‘... are green’ functions differently from that of one ending ‘... are 1,000’. In the former, the predicate applies distributively, that is, as holding good of each object to which the subject-term applies; in the latter, it applies, not to each of them, but to them collectively. This naturally leads to Mill’s proposal that the property denoted by the number-word attaches to a composite entity denoted by the subject-term, an aggregate or, in Mill’s own terminology, an agglomeration. This is really the only serious rival to Frege’s own semantic analysis of such sentences. It is everybody’s first thought, and arises from being too readily impressed by the grammatical similarity between singular and plural subjects. On this naive view, when the predicate applies distributively, the plural subject simultaneously denotes *each* of the objects to which it applies: the sentence states that the predicate holds good of each such object. But, when the predicate applies collectively, as when we say, ‘Dodos are extinct’, or, ‘Gorillas are becoming rare’, we have something resembling a singular sentence. The predicate is not intended to apply to each individual object: it is not of a *kind* which it would make sense to apply to any individual object. Rather, it applies to (past or present) dodos or gorillas *as a whole*; and so, trying to analyse the plural subject by analogy with a singular one, we come up with the idea of treating it as standing, in such a sentence, for a single composite object made up out of the individual ones. Such was Mill’s theory; and so did many think of the matter in Frege’s time.

Frege tackles this in § 23. It has two fundamental weaknesses. First, for an aggregate to exist, there must be some relation between its parts in virtue of which they cohere; but, to give the number of objects of a certain kind correctly, there need be no particular relation between them at all. ‘Do we have to gather all the blind in Germany into an assembly for the expression “the number of blind people in Germany” to have a sense?’ is how Frege satirically puts the point. This may fail to impress those who think in terms of disconnected ‘sums’ of matter; they may find it harder to answer Frege’s question whether there are really any aggregates of proofs of a theorem or of events. But the critical objection to the theory is that, to assign any definite number to an aggregate, we should have to know what it was to be considered an aggregate *of*; and there is no one way to regard an aggregate as composed of parts. ‘A bundle of straw can be split up ... by separating it into individual straws, or by making two bundles out of it’; and this is to ignore the fact that each straw consists of cells or of molecules. A plural subject does not, in any context, denote a whole made up of parts; and so a number is not a property of any such composite object.

The colour blue, Frege remarks in § 22, 'belongs to a surface independently of any choice of ours . . . ; our way of regarding it cannot make the slightest difference' to it. This, he says, constitutes 'an essential difference between colour and number'. In this section, he hammers home the point that 'our way of regarding' something does make a difference to the number to be assigned to it. This is essentially the same point as that concerning aggregates: it is not enough to know how to delineate the totality to be numbered; it must also be known what it is to be regarded as being a totality of. In Frege's famous example:

If I give someone a stone with the words, 'Find the weight of this', I have thereby given him the whole object of his investigation. But if I place a pile of playing cards in his hands with the words, 'Find the number of these', he does not know whether I wish to discover the number of cards, or of complete packs, or of complete suits. I have not yet completely given him the object of his investigation by putting the pile into his hands; I must add a word – 'cards', 'packs' or 'suits'.

(The example has here been slightly altered: instead of 'suits', Frege wrote 'point-values for Skat'.) Likewise, by adopting different ways of regarding it, 'I can conceive of the *Iliad* as a single poem, as twenty-four Books, or as a large number of lines'. The point is repeated in § 25, in response to Mill's observation that two horses 'are a different visible and tangible phenomenon' from one horse: 'one pair of boots may be the same visible and tangible phenomenon as two boots; here we have a difference of number to which no physical difference corresponds.'

Subjective and objective

All this naturally leads to the suspicion that the number assigned depends upon some subjective way of conceiving the matter; not, indeed, on the part of those, like ourselves, who already know the answer, but on that of anyone who had not thought about the matter before, or had thought of it only ineptly, and who was innocent of semantic theories based on syntactic analyses after the pattern of mathematical logic, which is to say of a Fregean logic. Frege is in fact able in § 25 to quote Berkeley as arriving at the conclusion that 'number . . . is nothing fixed and settled, really existing in things themselves', but 'is entirely the creature of the mind'. The illusion that the number to be ascribed depends upon the subjective choice of a way of regarding the matter is due solely to our having selected the wrong subject for the ascription, namely an aggregate or the like. Just because the aggregate does not, by itself, determine what it is to be taken as an aggregate of, that is, what is to count as a single constituent and what as two distinct constituents, we are driven, so long as we conceive of the number as attaching to the aggregate, to suppose that some subjective conception is needed to determine which number is to be ascribed

to it; but all that shows is that we ought not to have taken the aggregate as the subject of the ascription in the first place. The subjectivist account is sufficiently refuted by the observation that an ascription of number is as objectively true or false as any proposition can be. As Frege says in § 26, 'the botanist means to say something just as factual when he states the number of petals of a flower as when he states their colour. The one depends as little on our arbitrary choice as does the other. There is, therefore, a certain similarity between number and colour; but it consists, not in their both being perceptible by the senses in external things, but in their both being objective.'

That is enough to resolve the question whether or not number is in part subjective; yet Frege devotes a good deal of space to discussing the matter in §§ 26 and 27. He does so in order to take the opportunity to state his views on a topic essential to his philosophy of mathematics, and, indeed, to his philosophy in general: the opposition between the objective and the subjective.

One of the most evident features of mathematics is its objectivity: the validity or invalidity of its proofs and definitions is determinable to the satisfaction of all. Sometimes, indeed, this objectivity appears to be breached: disputes arise over the legitimacy or otherwise of this or that method of proof. But such disputes are conducted on the assumption that they can be resolved, and are not to be left as matters of taste; and, despite the still unreconciled schism between classical and constructive mathematicians, they usually are eventually resolved. Unless, therefore, a philosopher of mathematics is content, with Brouwer, to deny this objective character, he must come to terms with it. Those who hold mathematical structures and mathematical objects to be mental constructions, as Dedekind did in Frege's day, obviously have the hardest task in doing so. A mental construction is effected, in the first instance, by a single individual, not by observable operations, but in the privacy of his own consciousness. It therefore becomes necessary, for one who takes this view, to maintain that such constructions can be communicated to others and faithfully reproduced in their consciousness, so that all may judge alike of propositions relating to them and reasoning concerning them. Mathematics, then, belongs, according to such philosophers, to the realm of the intersubjective: that which is common to all, but owes its existence solely to our mental activity.

Frege's view left no place for a category of the intersubjective, intermediate between the wholly objective and the radically subjective. For him, if something is common to or accessible by all, it must be independent of all; conversely, something whose existence depends upon the consciousness of any one must be private to that one, and not communicable to others. This is the primary source of his opposition to psychologism, the attempt to explain the meanings of the logical operators, or of mathematical propositions, or meaning in general, in terms of internal mental operations. If the meaning of a proposition essentially involved anything interior to consciousness, then, according to Frege, it

could not be conveyed, at least wholly or with assurance: we should have no way of knowing whether what had been aroused in the mind of the one to whom the attempted communication had been made was or was not the same as the original in the mind of the one trying to communicate it. Even if such a proposition could be communicated, there could be no common basis for determining its truth or falsity: if it appeared true to one and false to another, they could merely acknowledge their difference, but not resolve it. For that reason, psychology must be barred from logic and from mathematics: the only result of its intrusion would be the dissolution into inextricable subjectivity of what should be objective and the same for all.

An analysis of a concept and a description of the psychological operations necessary for attaining it are two quite different things. Only the former is relevant to the justification or the proof of mathematical propositions: it can be appealed to in a proof, whereas the psychological description is impotent to yield any mathematical conclusions.

A distinction needs to be drawn which could not be drawn in *Grundlagen* in the absence of the sense/reference distinction. In Frege's middle period, he always contrasts the senses of expressions, the thoughts expressed by sentences, with contents of consciousness such as sensations and ideas. The former are in themselves communicable and therefore objective, consequently not depending for their existence on our grasping or expressing them or judging them true or false; the latter are subjective and hence essentially incommunicable. In the late essay 'Der Gedanke', a breach is indeed made in the thesis of the communicability of senses; Frege there maintains that the pronoun 'I', when used in soliloquy – not when speaking to another – expresses a sense that only the thinker can grasp. Before this, however, he always contrasted senses with ideas as not being contents of consciousness and as accessible to all: in grasping a thought, the mind lays hold of what exists independently of the process of grasping it, and may therefore be the same for all who do grasp it. A sense which is in itself objective may, however, relate to something subjective, as when we speak of our own sensations: the sense is objective, but the referent is not. For it to be possible even to contradict an assertion, the thought denied to be true must be the same as that asserted, and recognisably so; hence, together with their constituent senses, all thoughts must be objective, and it is only they that can meaningfully be characterised as true or false. But, for the truth-value of a proposition to be assessable by criteria common to all, the references of its components must themselves be objective: if it concerns an object that only one person can apprehend, or involves a predicate whose application only one person can determine, then only that person is in a position to judge it to be true or false.

In *Grundlagen*, § 26, however, Frege applies the term 'objective sense' only to an expression of which, in the middle period, he would say that its *referent* was objective, and discusses colour-adjectives in the light of that distinction.

We customarily recognise the colour by the sensation to which it gives rise in us; and this sensation is subjective. But Frege has already stated, earlier in the section, that colour is an objective quality, so that a botanist's description of a flower as having petals of a certain colour is a factual statement. In so far as we use a colour-word in such a way that it can be decided to the agreement of all whether it applies to a given surface or not, we are using it in an objective sense, to designate an objective quality, not a subjective sensation. Even a colour-blind person can grasp this sense, because he knows what determines the application of the word: criteria employed in the laboratory, or simply the common response of those with normal vision.

From the standpoint of Wittgenstein's discussion of a private language, Frege is committing the error of supposing that we attach both a subjective and an objective sense to the colour-word: we ordinarily judge it to apply in the objective sense to a physical surface by recognising that it applies in the subjective sense to the visual sensation. If, then, there is no such subjective sense, but only the objective one on which Frege insists, is it irrelevant to that objective sense that colours are observational properties? Frege's idea is that the colour-blind man can grasp the objective sense of 'red' or 'green' as well as anyone else; and, if that is so, the observational character of the colour so designated does not enter into the sense of the word. The colour-blind man understands the word 'red' in the way we all understand 'magnetic': he judges its application to an object by the effects that object has. Among these effects are those upon the normally sighted; but, for the colour-blind man, the normally sighted have the same bearing upon the sense of the word that iron filings do upon the sense of the word 'magnetic'. Since the objective sense must be the same for all, the normally sighted cannot construe it in any other way than the colour-blind; so the (*objective*) sense they attach to it takes account of their capacity to recognise the colour of something just by looking at it, but is indifferent to this being *their own* capacity.

The implausibility of this view may be relieved by a distinction Frege never drew: between the sense a word has in the language to which it belongs, common to many speakers, and an individual speaker's grasp of that sense, which may be erroneous and may, even when correct, rest upon some association or ability peculiar to himself. According to the explanations he gave in his writings after 1891, the sense a speaker attaches to an expression consists in the way in which its reference is given to him. That explanation, in itself, would allow sense to be wholly subjective, different speakers attaching different senses to the same expression, and only the reference being the same for all. For Frege, however, the sense of an expression is objective, being part of the thought communicated by a sentence containing it. This is because it is not enough for communication that two speakers associate the same reference with an expression: they must know, or at least be able to determine, that they do. It is likewise not enough that a proposition cannot in fact be true according

to one speaker's understanding of it, and false according to another's: each must judge of its truth and falsity by the same criteria, and acknowledge the same reasoning to that proposition or its negation as conclusive.

The matter is nevertheless more complex. The use of a language in communication requires that the speakers know the generally accepted criteria for determining the reference of an expression, and hence the truth-value of sentences in which it occurs. It does not follow that those criteria exhaust the manner in which its reference is given to any one speaker, or even that they always could do. The normally sighted user of a colour-word knows that his unaided judgements about its application count, for others, as a defeasible piece of evidence, and, as a speaker of the common language, is content himself so to treat them. But his mastery of the word is based upon his ability to recognise something as red or yellow just by looking at it; the sense of the word is given to him as the name of a colour with which he is familiar, and his knowing what a colour is rests upon his ability to match colours.

In *Grundlagen*, Frege did not yet have the conception of the sense of an expression as the way in which its reference is given; the example of colour-words is discussed in simpler terms. It is cited, not for its own sake, but to emphasise that that which can be judged by common criteria, and that *about* which it is possible to judge by common criteria, must be objective, and hence, according to his conception of objectivity, cannot be the product of human mental activity. Since both ascriptions of number and arithmetical propositions are judged by common criteria, numbers must therefore be objective in this strong sense.

Actual and non-actual

They are objective: but they are not 'actual'. § 26 does not serve only to draw the contrast between subjective and objective as Frege sees it; it also introduces a distinction, of great importance in his eyes, within the realm of the objective, that, namely, between what is and what is not actual (*wirklich*). In most contexts, the German adjective '*wirklich*' is properly translated 'real'; but Frege emphatically does not use it to mean 'real' as opposed to 'fictitious' or the like. It serves, rather, as his manner of distinguishing between what present-day philosophers usually call 'concrete' and 'abstract' objects, though his contemporaries were more given to speaking, ambiguously, about 'real' as opposed to 'ideal' ones. Frege does not use the term 'ideal', but his use of '*wirklich*' is in line with that terminology. An object is *wirklich* for him if it is a causal agent; in § 85 he speaks of restricting the actual (*wirklich*) to 'that which acts upon our senses, or at least engages in actions which may have sense-perceptions as their immediate or remote consequences'; the German words here translated 'acts' and 'actions' are cognate with '*wirklich*' as the English words are with 'actual'. The examples cited in § 26 of objects which are not *wirklich* are the

Earth's axis, the Equator and the centre of mass of the solar system. The Equator is in no way subjective, nor was it created by our thought; but it is not causally efficacious: you cannot trip over it, or claim to feel that you are crossing it. One might conclude from these examples that something that is not *wirklich* may yet be acted on, since the Earth's axis is subject to precession and nutation; but the conclusion should be drawn only with caution. In § 46 Frege rejects the idea that the number of inhabitants of the German Reich can change from year to year: it is merely that one number is the number of inhabitants of the German Reich at the beginning of 1882, and another number that of the inhabitants of the German Reich at the beginning of 1883; he says the same in the essay 'Was ist eine Function?' of 1904. So perhaps there is no such point as the centre of mass of the solar system, which changes position as the planets move: only one point that has that status at one moment, and another that has it at another moment.

However that may be, it is to the class of objects that are objective but not actual (*wirklich*) that numbers belong. The failure to recognise that something can be wholly objective without being actual leads to grievous errors. In the Preface to *Grundgesetze*, Frege stigmatises this failure as the root of the psychologistic conception of mathematics: if numbers are not actual, as on Mill's empiricist conception, they can only be, for one who makes this mistake, contents or creations of consciousness.¹ The recognition that there is no reason 'why what has an existence independent of anyone making judgements about it must be actual (*wirklich*), that is, capable of directly or indirectly acting on the senses' is fundamental to Frege's entire philosophy of mathematics.

§ 26 ends with Frege's characterisation of objectivity as 'independence from our experience, intuition and imagination and from the delineation of inner images from the memory of earlier experiences'. § 27 spells out the subjectivist consequences of the psychologistic interpretation of numbers: if numbers are ideas, each individual has his own arithmetic, and cannot dispute with anyone else whose arithmetic differs from his. Here Frege inserts his footnote, already discussed, complaining of Kant's failure to distinguish subjective from objective ideas, and records his own decision to use the word 'idea' in an exclusively subjective sense. The conclusion is that 'number is neither spatial nor physical, . . . nor subjective like ideas, but non-sensible and objective'. And, with that, Frege is ready to bring his critical preliminaries to a close: he has extracted several positive principles from the discussion; he is on the verge of giving his solution to the problem what number is and what it is ascribed to.

He does not bring it to a close, however. Before giving his own positive account, he interposes a lengthy but brilliant section demolishing the widely favoured abstractionist theory of numbers as sets of units.

¹ Vol. I, p. xviii.

CHAPTER 8

Units and Concepts

Numbers as sets of units

In the brief § 28 of *Grundlagen*, Frege rejects the conception of a number as a set or plurality, using the now familiar objection that it fits the numbers 0 and 1 particularly badly. That the number of objects of a given kind should be the set of those objects is sufficiently absurd to need no refuting: the section serves principally to introduce Frege's attack upon the widespread conception, common, as we have seen, to Husserl and Cantor, among many others, of a number as a set of featureless units. According to this theory, starting with a set of objects to be counted, we abstract from all the specific properties of those objects, thus mentally fashioning a purely abstract set whose members – units – have no properties whatever; that is the number of objects in the original set. Plainly, if we had started with any other set containing just as many objects as the first, we should by this means arrive at the same abstract set of units: such abstract sets thus have the essential characteristic we desire of numbers, that the number belonging to any set is the very same as that belonging to any other set that has, as we say, the same number of members.

What does 'one' mean?

It is this theory which Frege sets himself, in §§ 29 to 44, to refute: and he does so brilliantly, decisively and definitively.¹ The first part of his discussion, from § 29 to § 33, does not deal with the full-blown theory, and does not mention abstraction, but treats only of vaguer characterisations of a number as a 'multitude of units', as in Euclid's definition, which he begins by alluding to. What, then, is a unit? Is it an object that is one? But if the adjective 'one' denoted a property of objects, and so functioned as what, from § 38 onwards,

¹ Later works in which Frege repeats his critique of this abstractionist theory are 'Über den Begriff der Zahl' (1891–2), *Nachgelassene Schriften*, pp. 81–95, *Posthumous Writings*, pp. 72–86, *Über die Zahlen des Herrn H. Schubert* (1899) and pp. 589–90 of 'Antwort auf die Ferienplauderei des Herrn Thomae' (1906), the second of these with very heavy sarcasm; but none adds anything substantial to his refutation of it in *Grundlagen*.

Frege calls a 'concept-word', it would stand for a property possessed by every object whatever (§ 29): whereas

It is only in virtue of the possibility that something should not be wise that the assertion that Solon is wise obtains a sense. The content of a concept diminishes as its extension increases; if the latter becomes all-embracing, its content must vanish altogether.

It is even worse to suggest, with Baumann, that whether something is one or many depends on our way of regarding it (§ 30). In fact, we cannot admit such a sentence as 'Solon was one' as well-formed, save when understood as supplemented by some concept-word supplied by the context; and, if we did, we could not, from 'Solon was one' and 'Thales was one', infer 'Solon and Thales were one'.

It is useless to attempt to explain 'one' as standing for such a property as that of being circumscribed, self-contained or undivided. 'When we say that the Earth has one moon, we do not mean to specify that the Moon is circumscribed, self-contained or undivided' (§ 32); for the moons of Jupiter are circumscribed, self-contained and undivided as our own. If the word 'one' denoted a property of this kind, we should expect even animals to have some idea of it; but it is improbable that a dog 'has even a dim awareness of the common element in the cases in which he is bitten by *one* larger dog and in which he chases *one* cat' (§ 31). Still less does it mean 'thought of as undivided or indivisible'; this makes the application of 'one' subjective once more, whereas truth cannot be attained by thinking of things as they are not (§ 33). It follows that the notion is not, as Locke supposed, 'suggested to the understanding by every object without us, and every idea within', but that it is, rather, attained 'by means of those higher mental powers that distinguish us from the beasts'.

Abstractionism

The word 'unit' cannot, therefore, be explained as applied to objects in virtue of their possessing the property denoted by the word 'one'; for the word 'one' denotes no property. Frege quotes various writers (Schröder, Hobbes and Hume) as maintaining that units are to be considered as, or that they strictly are, identical with one another (§ 34). How does this come about, since, plainly, the actual objects to be counted must be distinct? Frege quotes his colleague at Jena, J. Thomae, as appealing to abstraction as the means by which the identity is achieved: he might, of course, have cited any one of a number of contemporary writers. We 'abstract from the peculiarities of the individuals in a set of objects' and, 'in considering separate things, disregard the characteristics by which those things are distinguished from one another'.

Frege does not here launch any general attack upon the whole idea of abstraction: he simply remarks that such a process will not yield either a set of units, or the number of things in the set, but only 'a general concept under which those things fall'; 'the things themselves do not thereby lose any of their peculiarities'.

If, for example, in considering a white cat and a black cat, I disregard the properties which distinguish them, I obtain, say, the concept 'cat'. If I now bring them both under this concept, and call them units, the white one still remains white, all the same, and the black one black. The cats do not become colourless as a result of my not thinking of their colours; they remain just as different as they were before, for all my resolving to draw no conclusions from their difference. The concept 'cat', which has been attained by abstraction, indeed no longer includes the peculiarities of either; but, just for that reason, it is a single concept.

It is fatuous to base on this remark, and other similar ones in *Grundlagen*, an onslaught on Frege as one of the chief nineteenth-century proponents of abstractionism, as do G. Baker and P. Hacker.² Frege was one of the earliest and most vigorous opponents of the doctrine of abstraction, at a time when it was generally taken for granted among philosophers: if his attack was still not as sweeping as it could or ought to have been, that is no ground for criticising him as an arch-abstractionist. To the extent that he admitted the notion of abstraction, that notion plays no role in any of the philosophical theses he was concerned to advance; it is no part of his argument in § 34 that we *do* arrive at a concept by this means, but only that *the most* we could so attain would be a concept. Such a criticism is not merely fatuous: it misses the point. We may consider three theses, in ascending order of strength:

- (1) it is possible to attain a radically new concept by contemplating a number of otherwise diverse objects falling under it;
- (2) the attainment of the new concept in case (1) is effected by abstracting from the properties differentiating the objects in question, i.e. by diverting the attention from them;
- (3) the operation of abstraction referred to under (2) can also generate abstract mental constructions, that is, abstract objects or structures of objects that lack all those properties abstracted from and have no others in their place.

It is true enough that, from other passages in *Grundlagen* (§§ 45 and 48), it appears that he did at that time accept thesis (2), regarding abstraction as one genuine means of concept-formation; but still no argument that he advanced

² Frege: *Logical Excavations*, Oxford, 1984, pp. 57–8.

depended on that belief. What, in *Grundlagen* and elsewhere, he was concerned to combat was thesis (3), which alone has a bearing on the philosophy of mathematics. His essential, and crucial, contention in *Grundlagen* was that abstraction is (at best) a means of coming to grasp certain general concepts: as a mental operation, it has no power to create abstract objects or abstract structures. Subsequently, in the review of Husserl, published in 1894, he rejected abstraction altogether. That is, he rejected not only thesis (3), but thesis (2) as well; his words are consistent with his continuing to accept thesis (1), but nothing he says hangs on that and there is no reason to assume that he still believed it. Thus he wrote, with heavy sarcasm, repeating the *Grundlagen* example:³

Since everything is an idea [on the view Frege is ascribing to Husserl], we can easily alter objects by directing our attention towards this and away from that. The latter is particularly effective. We take less notice of a property, and it vanishes. By causing one characteristic after another to vanish, we attain to ever more abstract concepts. Concepts, too, are therefore ideas, merely less complete ones than objects; they have only those properties from which we have not yet abstracted. Inattention is a highly effective logical force; hence, presumably, the absent-mindedness of scholars. Let us suppose, for example, there are sitting side by side in front of us a black and a white cat. We pay no attention to their colour: they become colourless, but are still sitting side by side. We pay no attention to their posture: they are no longer sitting, without, however, assuming a different posture; but each is still in the same position. We cease to attend to their places: they become devoid of position, but continue to be apart from one another. We have thus, perhaps, attained from each of them a general concept of a cat.

Whereas in *Grundlagen* Frege had allowed that the concept 'cat' might be attained in this way, here he mocks even that application of the operation of abstraction, by repeated application of which, he says, 'every object is transformed into an ever more bloodless ghost':

Whereas, on my view, bringing an object under a concept is merely the recognition of a relation which already obtained beforehand, here the objects are essentially altered thereby, so that the objects brought under the same concept become more similar to one another.

There could not be a plainer rejection of thesis (2), and therewith of the whole notion of abstraction, of which Baker and Hacker excoriate Frege for being a leading proponent.

³ Review of Husserl, pp. 316–17.

Are units identical?

In *Grundlagen*, unlike the review of Husserl, Frege expends little energy on contesting the whole idea of abstraction as a means of constructing abstract entities by mental operations, and devotes little attention to the process by which a set of units is generated; instead, he concentrates on the product, that is, on the nature of the set of units. He poses a fundamental dilemma for the conception of numbers as abstract sets of featureless units, namely whether the units are identical with or distinct from one another. The point of interpreting numbers as sets of units, rather than taking the number of objects of a given kind simply to be the set consisting of those objects themselves, is obviously to guarantee that the number will be independent of the particular objects counted, being determined, as it ought to be, solely by how many of those objects there are: if, say, there are just as many spoons as forks on the table, the number of spoons on the table will be the very same abstract entity as the number of forks on the table. This requires that the set of units arrived at by abstraction from the set of spoons shall be the very same set of units as that arrived at by abstraction from the set of forks. It seems to be possible to guarantee this only if no trace of individuality is retained by the units: those derived by abstraction from the spoons must be identical with those derived by abstraction from the forks. This can be so only if the operation of abstraction strips the original objects of all their properties: for the spoons, or, rather, what had originally been spoons, could hardly retain features differentiating them from one another if they could no longer be differentiated from what had been forks. That is one reason why Frege is able to quote so many writers as maintaining the strict identity of all units with each other.

The conclusion poses an obvious difficulty for the theory; for, as Frege says, 'we cannot succeed in making different things identical simply by operations with concepts; but, if we did, we should no longer have *things*, but only a single thing' (§ 35). And he quotes W.S. Jevons as saying, 'It has often been said that units are units in respect of being perfectly similar to one another; but though they may be perfectly similar in some respects, they must be different in at least one point, otherwise they would be incapable of plurality'. If every unit is identical with any (other) unit, there can only be one unit. As Frege summarises the problem (§ 39), 'if we try to make the number originate from the combination of distinct objects, we obtain an agglomeration comprising the objects with just those properties that differentiate them; and that is not the number. If, on the other hand, we try to form the number by a combination of identicals, this constantly coalesces into one, and we never reach a plurality.'

Frege reviews the efforts of Jevons and others to wriggle out of the difficulty. If the units are thought of as differing only in their spatial or temporal positions, the application of number to what is non-spatial and non-temporal is once

more rendered impossible (§ 40). In any case the manoeuvre is of no avail; for, even if points of space, for instance, differ only in their relations to one another, they still differ, so that numerically equivalent sets of points may still be quite distinct (§ 41). Jevons proposes to regard the distinction between one unit and another as consisting merely in 'the empty form of difference' (§ 44), which is to say that they differ, but not in any respect. Even if that were acceptable, it still would not help: if the units differ from one another at all, then there may be distinct but numerically equivalent sets of units, and the purpose of the entire theory is frustrated. The difficulty becomes particularly acute when we consider the addition and subtraction of numbers. If numbers are sets of units, it lies to hand to interpret addition as set union and subtraction as set difference. Now numbers are specific entities (§ 38):

We speak of 'the number one', and indicate by means of the definite article a single, determinate object of scientific enquiry. There are not distinct number ones, but only a single one. In 1 we have a proper name, and, as such, it is as incapable of a plural as 'Frederick the Great' or 'the chemical element gold' . . . Only concept-words can form a plural.

Hence, if units differ from one another, and numbers are sets of units, each number must be a specific set of particular units. Either any two numbers are disjoint, or (as Jevons appears to suppose) larger numbers include smaller ones. In either case, it becomes impossible to explain how addition or subtraction can yield the right set of units to constitute the number that we know results from the arithmetical operation. Without a loophole for escape, Frege has shown how, even without calling in question the magical operation of abstraction, one may reduce the entire theory to ruins.

The solution

With § 44 Frege's critique of untenable views comes to a triumphant end (though there is to be a supplementary critique of postulationism in §§ 92–103). It remains to provide positive answers to the outstanding questions, namely:

What is a number a number of, that is, what is number ascribed to?

What is (cardinal) number in general?

What, specifically, are the numbers 0 and 1?

What is the relation between a natural number and its successor?

In § 46, the celebrated answer to the first of these questions is given:

'die Zahlangabe enthält eine Aussage von einem Begriffe.' Annoyingly, this terse formulation is difficult to translate. A 'Zahlangabe' is what we have been calling an 'ascription of number', namely a sentence beginning with an expression of the form 'There are n . . . ' (where ' n ' represents a number-adjective). 'Angabe' is more accurately rendered 'statement' than 'ascription'; the latter has been preferred here as more readily comprehensible. To translate the verb as 'contains', which is what it literally means, is misleading, carrying as it does the connotation 'contains among other things': Frege merely means to indicate what, according to him, is the content of such a form of words. Accordingly, we may begin our rendering of the slogan by 'The content of an ascription of number is . . . '. How, then, is 'eine Aussage von einem Begriffe' to be translated? Austin's version, 'an assertion about a concept', imports a suggestion that is not present, and omits one that is. By 'Aussage', Frege does not mean to single out assertions, as opposed to questions, commands, etc.; Friedrich Waismann, misled, apparently, by the word (which can indeed be used to mean 'assertion'), actually criticised Frege, quite erroneously, for restricting his account of number-words to assertions, and not providing for their use in interrogative and imperative sentences.⁴ Frege uses 'Aussage' here to mean 'predication': he intends to convey that, in an ascription of number, something is predicated of a concept, in analogy with the sense in which the sentence 'Julius Caesar was ruthless' is used to predicate something of the individual, Julius Caesar. Unfortunately, the English phrase 'a predication of a concept' is most naturally understood as applying to the act of predicating a concept of some object. The best we can do, therefore, is to render the slogan as 'The content of an ascription of number consists in predicating something of a concept'.

So, then, there is Frege's solution: what a number is ascribed to is a concept. When we regarded it as ascribed to a complex, an aggregate, it seemed that the number to be ascribed depended on our subjective way of regarding it: as one copse, or as five trees; as four companies, or as five hundred men. But there is nothing subjective about it: it is the concept, *copse* or *tree*, *company* or *man*, which we invoke in the ascription of number, that determines objectively which number it must be (§ 46). For a concept is not a subjective idea, but is as objective as an object: independently of anything we may think or imagine, we predicate something of a concept as truly or as falsely as we predicate something of an object (§ 47). An aggregate does not of itself determine how it is to be split up into components: it is the *concept* that determines that unambiguously. This is why number is so widely applicable (§ 48). All objects, of whatever kind, fall under one concept or another: hence objects of all kinds can be numbered. For a number to be ascribed to a given concept, there need be no physical relationship between the objects falling under it, nor do we need to perform any mental operations upon our ideas of those objects: the

⁴ *Einführung in das mathematische Denken*, 2nd edition, Vienna, 1936, p. 81; English translation by Theodore J. Benac, *Introduction to Mathematical Thinking*, New York, 1951, pp. 114–15.

concept of itself performs the only function of gathering them together or of singling them out that is needed (§ 48). Of course, to obtain a determinate number, we must consider, not the general concept *copse* or *tree*, unless we are wanting to give the number of all copses or trees in the universe: the relevant concept is that of a copse or a tree in such-and-such a place at such-and-such a time (§ 46).

Frege's notion of a concept might seem at first like the traditional notion of a universal, which can be predicated of particulars, but can also have something predicated of it; but the two notions differ crucially. On the traditional conception, if we are to predicate anything of the universal, the predicate which expresses it when it is predicated of particulars must be transformed into an abstract singular term: but Frege's idea is that an ordinary form of sentence, in which the concept-word occurs in predicative form, as a general term, plural or singular, may serve to predicate something of the concept for which it stands. Frege's notion is very much broader than the traditional notion of a universal. The general form of a judgeable content which treats of an object *a* is '*a* falls under the concept *F*', he tells us in § 70. Taken together with the *Begriffsschrift* doctrine of the extraction of concepts from judgeable contents, which allows any proper name or other singular term, occurring anywhere in a sentence, and, when it occurs more than once, at any selected number of places, to be regarded as variable, this yields a very wide conception of what is to count as standing for a concept, embracing far more than can be represented by any abstract noun-phrase.

Frege's doctrine concerning concepts does not fully emerge in *Grundlagen*; we have to go to the writings of his middle period to grasp it properly. Certainly, some elaborations were new: he would not have said, at the time of *Grundlagen*, that concepts were functions from objects to truth-values, and is unlikely to have thought of them, as Baker and Hacker suppose, as functions from objects to judgeable contents; nor, of course, did he distinguish between concepts and the senses of concept-words. For all that, much of what he said about them after 1890 must already have been present in his mind when *Grundlagen* was written. An adherent of the traditional doctrine of universals and particulars would certainly agree that we cannot understand an abstract term for a universal unless we take it as standing for something that can be predicated of particulars; the role of the universal as predicate is prior to its role as subject. It was not part of that traditional doctrine, however, to hold that any intelligible predication of some characteristic to a universal must be able to be expressed by a sentence in which the universal is represented by a predicative expression or general term. It would be generally agreed that the analogue does hold for mathematical functions. We can intelligibly say, e.g., 'The sine function is everywhere differentiable', only because the same thought can be expressed by a sentence invoking only the functor 'sin ()', containing an argument-place. It is for this reason that it is impossible to say of a function

what can meaningfully be said of a number, or conversely; and Frege was certainly of the same view in respect of concepts and objects. He later went much further, indeed, denying that such an apparent singular term as 'the sine function' was well-formed at all. Only an incomplete or 'unsaturated' expression – one with an argument-place – could stand for a function or a concept; hence a functor or concept-word ought never to appear without its argument-place.

This of course led to the paradox which Frege attempted to dismiss, and failed to resolve, in 'Über Begriff und Gegenstand', to the effect that the concept *horse* cannot be a concept, since the phrase 'the concept *horse*', as a singular noun-phrase with the definite article, can only stand for an object. From the remark already quoted from *Grundlagen*, § 70, that '*a* falls under the concept *F*' is the general form of a proposition about an object, it is apparent that Frege was as yet oblivious of this difficulty. He has in mind sentences representable symbolically as ' $F(a)$ ', which, according to his doctrine, include all sentences containing a singular term, at least when viewed in a particular way. He emphatically does not mean that such sentences invoke a relation, that of falling under, obtaining between objects and concepts. The concept is predicative by nature, requiring an argument for completion, and hence of itself couples, as it were, with the object to make a complete judgeable content. It is sometimes asserted that the metaphor of saturation was not used by Frege before 1891: in fact, though it does not occur in *Grundlagen* itself, it can be found in his letter to Anton Marty, written in 1882 when the book was nearly finished, where he says:

A concept is unsaturated, in that it requires something that falls under it; hence it cannot subsist by itself alone. That an individual falls under it is a judgeable content, within which the concept appears as the predicate, and is always predicative. In this case, in which the subject is an individual, the relation of subject and predicate is not a third thing added to the two of them, but belongs to the content of the predicate, which is what makes the latter unsaturated.

The denial that the relation of subject to predicate, or of object to concept, is a third ingredient seems to be contradicted by Frege's representation of, say, 'Odysseus was set ashore at Ithaca' as saying of Ithaca that it falls under the concept 'place where Odysseus was set ashore'; but the point of that representation is simply to call attention to one possible analysis of that sentence, not to make explicit an element only tacitly alluded to. As already observed, the same holds good for all the jargon in *Grundlagen* about numbers belonging to concepts and the like.

The ban on expressions like 'the sine function', and, ultimately, 'the concept *horse*' – putative proper names of functions and of concepts – may be thought excessively severe; it would carry with it, naturally, a ban on second-level predicates masquerading as first-level ones, such as 'is continuous' or 'is

transitive' (applied to a function or a relation). Certainly no one, not even Frege, has attempted to observe it when using natural language. But Frege's purpose – admittedly obscured by the use of the jargon – was to analyse sentences – expressions of judgeable contents – *as they stand*. When we predicate something of a concept, we do not need, and in fact ought not, to transform the expression for the concept into the grammatical subject. Such a transformation is indeed effected by Frege's jargon, which serves to emphasise that a *predication* is involved by presenting it in the form most familiar to us for predications, namely that in which the subject of the predication is grammatically a singular term: but the point of the jargon is not to assimilate sentences predicating something of a concept to ones predicating something of an object, but to highlight the *analogy* between them, despite the obvious difference of verbal form and the difference of logical level, on which Frege insists as firmly as on the analogy. It is possible to make such second-level predications only because our language, as we ordinarily employ it, allows us to form sentences embodying those predications, in which the expression for the concept remains predicative in form, and hence clearly recognisable as an expression for a *concept*. It was Frege's clear simultaneous recognition of the analogy between 'The Danube is long' and 'There are five sheep in the meadow' and of the difference of level between them that enabled him to introduce a type unknown to Kant, that of properties of concepts, or, better, concepts of second level or order (*Grundlagen*, §§ 46 and 53). We ascribe properties to (first-level) concepts, as well as ascribing properties to objects; but the kind of property a concept can have is utterly different from a property possessed by an object, and expressed by a form of words differing radically in logical character. If we observe the injunction never to use, as an expression for a concept, one that is complete and lacks an argument-place, then it becomes impossible even to try to say of a concept what can be said of an object, or conversely.

It is the usual practice nowadays to describe the semantic value of a one-place predicate as being a set, viz. a subset of the domain, rather than as an unsaturated entity like a concept in Frege's sense. What is here meant by a 'set' is what Frege normally called a 'class'; except occasionally in late writings dating from the time when the phrase 'set theory' had become standard, Frege always used 'set' to mean an aggregate *made up out of* individuals – what partisans of the calculus of individuals call a 'sum'. A set, in this sense, has parts, rather than members, and the part-whole relation is transitive; there therefore cannot be such a thing as an empty set, and there can be no distinction between an object and a set consisting wholly of that object.⁵ A class, on the other hand, is for Frege the extension of a concept:⁶ it is the *concept* which determines what is and what is not a member of the extension,

⁵ See the review of Schröder (1895), pp. 433–7.

⁶ *Ibid.*, p. 455.

and thus may rule out from membership the member of a member. As with numbers, only a concept can articulate the class into its members in a determinate way. It is because a concept can be empty that there is such a thing as the empty class; and it is because the extension of a concept need not itself fall under that concept that we can distinguish a unit class from its sole member.

An extension of a concept is not, indeed, the same thing as the concept. This is not because concepts are intensional rather than extensional. Frege identified himself with extensionalist logicians, as against the intensionalist ones,⁷ and declared that the relation between concepts analogous to that of identity between objects was that of being co-extensive.⁸ The analogy can only lie in the principle of extensionality: whatever holds good of a given concept must hold good of any co-extensive one. The difference is, rather, one of level: classes are objects, and, as such, are denoted by singular terms. They can therefore themselves fall under first-level concepts, and hence belong to the extensions of such concepts. But the notion of a concept is prior to that of the extension of a concept: we can only form the notion of a class via that of a concept which determines what is and what is not a member of it; a class can be given only as the extension of a concept. That is why, in the formal system of *Grundgesetze*, Frege takes as primitive, not, as in our conventional systems of set theory, the symbol for the membership relation, but that for class abstraction. (Strictly speaking, it is a more general abstraction operator, forming expressions for value-ranges – functions in extension; but the generalisation may be passed over for the present.) Membership is, in *Grundgesetze*, a *defined* relation: *a* is a member of *b* if there is some concept *F* such that *a* falls under *F* and *b* is the extension of *F*. Here is one of the many places where Frege found second-order quantification indispensable. Thus, from Frege's, surely correct, viewpoint, we do not dispense with the notion of a concept by making the semantic value (reference) of a predicate a class: we still need the concept to explain what a class is and what it is to belong to it. If we explain a sentence of the form '*F(a)*' as being true just in case the object denoted by '*a*' belongs to the class associated with '*F*', we are merely saying that the object falls under some concept with whose extension '*F*' is associated: we could more simply associate the concept directly with the predicate, and say that the object falls under that concept. Indeed, we shall do better to say the latter, because the object which constitutes the extension does not really enter into the matter at all. By bringing it in, we obscure the unity of the proposition, whose two parts fit together because they were made to do so. A concept is predicative by nature, which is to say that it is constituted by the fact that

⁷ See the review of Schröder, p. 455, and the unpublished 'Ausführungen über Sinn und Bedeutung', apropos of Husserl's review of Schröder, *Nachgelassene Schriften*, p. 133, *Posthumous Writings*, p. 122.

⁸ See the review of Husserl, p. 320.

certain objects fall under it and others do not. Class-membership is a relation between objects which ought to be expressly mentioned whenever it is invoked. The relation between an object and a concept which obtains when the former falls under the latter, on the other hand, is what we might call a formal relation (in analogy with Wittgenstein's notion of a formal concept): it does not need to be, and is not, invoked in an ordinary predication, which is why there is no relation that fails to be symbolised in the form ' $F(a)$ ', and why an explanation of the truth-grounds of such a proposition in terms of concepts is preferable to one in terms of classes.

Fregean semantics undermines the superficial similarity between singular and plural. A grammatically singular noun-phrase may of course be functioning as a concept-word, and will always be so functioning when preceded by the indefinite article (*Grundlagen*, § 51): but a plural noun-phrase, even when preceded by the definite article, cannot be functioning analogously to a singular term. There are, of course, complex objects; but their continued existence depends on the maintenance of some relation between their components.⁹ If the troops cease to obey their officers, the army dissolves, with not a man lost; if the bicycle falls to bits, there is no longer a bicycle, though all the parts are there; even a pile of dust ceases to be when the wind scatters the particles. But a plural subject of predication or ascription cannot stand for any such composite object, both because it presupposes no relation between the objects alluded to, and because it determines which those objects are in a way in which no composite object is uniquely articulable into components. There is no such thing as a 'plurality', which is the misbegotten invention of a faulty logic: it is only as referring to a concept that a plural phrase can be understood, because only a concept-word admits a plural. But to say that it refers to a concept is to say that, under a correct analysis, the phrase is seen to figure predicatively. Thus 'All whales are mammals', correctly analysed, has the form 'If anything is a whale, it is a mammal', and 'The Kaiser's carriage is drawn by four horses' the form 'There are four objects each of which is a horse that draws the Kaiser's carriage' (§§ 47 and 46). On this analysis, no one has subsequently found an improvement, the only plausible variation being that which would substitute, say, 'any organism' for 'anything' in the first and 'organisms' or 'animals' for 'objects' in the second, importing an explicit circumscription of the domain into the quantifications.

⁹ In his letter to Russell of 28 July 1902, Frege discusses this notion of a composite object, under the name of a 'whole or system', using just this example of an army; see G. Frege, *Wissenschaftlicher Briefwechsel*, ed. G. Gabriel, H. Hermes, F. Kambartel, C. Thiel and A. Veraart, Hamburg, 1976, p. 222, or the English translation by Hans Kaal, *Philosophical and Mathematical Correspondence*, ed. B. McGuinness, Oxford, 1980, p. 140.

The number of red things

There is, however, a grave defect in Frege's answer to the question 'To what is a number ascribed?'. At the beginning of the 1885 lecture 'Über formale Theorien der Arithmetik', he said that 'nothing is really required' for a number to be assigned to things of a certain kind 'save a certain sharpness of circumscription, a certain logical completeness'. In *Grundlagen*, § 54, he recognises that not all concepts have this feature. The counter-example he there cites is the concept 'red'. Such a concept does *not* determine what is to count as a single object falling under it; the totality of red things in the room is, in this respect, no better than an aggregate. Asked to count the red things in the room, we do not know how to begin: if, say, the wallpaper pattern has a red background, are we to count the background as one red thing, or the connected red regions on the wallpaper as a large number of red things, or what? If a red-headed man is present, is the hair of his head one red thing, and each eyebrow another, and so on, or should we count the individual hairs? We are in a greater difficulty than when handed a pile of playing cards with the instruction, 'Count these'. Frege's response is inadequate: he says that 'to such a concept no finite number belongs'; but the fact is that there is no infinite number, either. Quite unlike the question, 'How many natural numbers are there?', the question, 'How many red things are there in this room?' has no answer, at least if there is anything red in the room; even to cite a transfinite number in answer to a question 'How many?', the concept requires 'a certain sharpness of circumscription'. Frege is at fault, not merely in giving no adequate response to the difficulty in *Grundlagen*, but in never reverting to a matter that called in question his solution to the problem to what a number is, in general, to be ascribed.

The fact is that he had not the resources to resolve the difficulty within the framework of his logic. It is not the vagueness of the predicate 'red' that causes the trouble. Assume it to be being used only of opaque surfaces, and its sense to have been so sharpened that, of every such surface, it is determinate whether or not it is red all over: still there is no saying how many red things there are in the room. And yet, given Frege's definition of 'the number of *F*s', it is perfectly easy to prove in his logic that, for any concept *F*, there is a determinate number of objects falling under it. By hypothesis, each object is, determinately, either red or not red: so what can stand in the way of there being a specific number, finite or transfinite, which is that of all the objects which are red? The difficulty arises from the fact that a Fregean semantics – a classical semantics for a language with the structure of standard (higher-order) predicate logic – assumes a domain already determinately articulated into individual objects. Some objects may be parts of other objects, just as one rectangle may be part of a larger rectangle; but this does not prevent them from being distinct objects, so that both part and whole are to be counted, if both fall under the

relevant concept; there is no difficulty in counting the rectangles in some given diagram. But the application of the predicate 'red' does not presuppose or depend on any prior articulation of the candidates for its application into discrete (even if overlapping) objects. This fact is in turn due to the existence of a level of language below and prior to that which can be regimented by the syntax of predicate logic, a level at which there is no reference to objects, but only ostension: when, pointing to some surface, I say, 'This is red', I am not referring to an object, because I have, and need, no criterion for recognising what I point to as the same again (§ 62), or for determining whether what someone else is pointing to at the same moment is or is not the same thing as that at which I was pointing. That does not mean, indeed, that my statement cannot be contradicted. But, if you say, 'You are wrong: that is not red', the first question to be settled between us is not whether you are pointing at the same thing (red or otherwise) as I was, but whether what you are pointing at is the same colour as what I was pointing at; if it is, you were genuinely contradicting me, and, if it is not, our statements can be reconciled. A concept like 'somnolent' or 'spherical', by contrast, is applied only to determinate objects, and thus does not belong to the most primitive level of language, but to its second level, which is representable by a quantificational syntax; and it can therefore be intelligibly asked how many somnolent or spherical things there are in a room at a given time. It is only to such concepts that numbers can be ascribed. A predicate like 'red', belonging to the most primitive part of language, can still be used at the next level, and applied to determinate objects; but, because it does not presuppose any articulation of reality into separate objects, we cannot intelligibly ask what number attaches to it.

Aggregates and concepts

Bell's defence of Husserl's *Philosophie der Arithmetik* against Frege's criticisms interested us in Chapter 2 because it helped to illuminate the issue of psychologism. It also bears on Frege's doctrine that it is to concepts that numbers attach. In his book,¹⁰ Bell goes so far as to defend, against Frege's criticism in his review,¹¹ Husserl's contention that 0 is not a number, on the ground that 'nought' is a negative answer to the question 'How many?';¹² he balks, though, at concurring with Husserl's denial of that status to 1, on an analogous ground. Bell misses Frege's point, however; both he and Husserl have the matter the wrong way round. If we deny that there is such a number as 0, we can and must hold 'none' to be a negative answer to 'How many?'. The same goes for 1 and for Aleph-0, vis-à-vis the answers 'one' and 'denumerably many'; the scholastics held it to be a priori impossible that the human race

¹⁰ D. Bell, *Husserl*, London, 1990, p. 70.

¹¹ Review of Husserl, pp. 327–8.

¹² E. Husserl, *Philosophie der Arithmetik*, Halle, 1891, p. 144.

had always existed, because there would then be no number that was the number of all men there had ever been. But Frege's point was that, by treating 0 and 1 (and Aleph-0) as numbers, we run into none of the antinomies that result from treating 'never' as the name of a time or 'nobody' as the name of a person. We therefore do not *have* to hold that 'none' is a negative answer, and so cannot infer, from the premiss that it is, that 0 is not a number. There are cases in which we *should* run into antinomies. One known to Frege is the number of red things in the room; another, not known to him, is the number of cardinal numbers.

Bell believes – falsely, as we saw in Chapter 2 – that Husserl, having explained the genesis of the concept of number, had a further explanation to give, namely of what the numbers are, and he attempts, on Husserl's behalf, to reconstruct it; but the attempt yields no clear explanation. Bell boldly claims superiority for Husserl's notion of an aggregate (*Inbegriff*) or plurality (*Vielheit*) over Frege's notion of a concept (*Begriff*); according to him, Husserl's use of the former notion enabled him to close the lacuna in Frege's semantic theory, its inability to treat plural terms in analogy with singular ones.¹³ For the notion of an aggregate to perform the task assigned to it, however, the features which distinguish it from that of a concept, as understood by Frege, and on which its superiority to the latter might be claimed to rest, are progressively jettisoned by Bell in favour of the features of concepts. Thus although an aggregate is a whole made up of parts,¹⁴ and the part-whole relation is transitive, its articulation into members is determinate¹⁵ – just the very feature that makes Frege think it essential to take a concept as that to which a number attaches; a member of an aggregate is a part of it, but not all its parts are members. Again, the existence of an aggregate does not, as Frege supposes, depend on any relation's objectively obtaining between its members.¹⁶ Frege, discussing Mill in *Grundlagen*, § 23, had assumed, surely rightly, that the constituents of a Millian aggregate or agglomeration must be related by physical proximity; but Bell correctly explains that, for Husserl, the unity of an aggregate arises purely from a *psychological* act of 'collective combination'.¹⁷

Although Husserl allows that membership of an aggregate is frequently determined by possession of a common property, and that it is always possible to specify a concept under which fall all and only the members of a given aggregate,¹⁸ he is emphatic that Frege was wrong and he is right in thinking that ascriptions of number relate to aggregates, not to concepts: '*The number therefore attaches, not to the concept of the objects being counted, but to the aggregate*

¹³ Op. cit., pp. 63ff.

¹⁴ Op. cit., p. 38.

¹⁵ Ibid., p. 78.

¹⁶ Ibid., pp. 50–1.

¹⁷ Bell, op. cit., pp. 48–50; *Philosophie der Arithmetik*, pp. 13–16, 76–81.

¹⁸ *Philosophie der Arithmetik*, pp. 184, 188–9; see Bell, op. cit., p. 77.

of them.¹⁹ Despite this, his explanations of the notion of an aggregate are tantalisingly inadequate; this is due especially to his concentrating in the first instance on aggregates of objects of perception. It is hard to discover which features aggregates are supposed to retain in a more general context, and hence whether Bell's observations about the notion are faithful to Husserl's conception or not. Even supposing them to be so, it is natural to wonder if any difference is left between an aggregate and a concept. Bell insists that an aggregate is of a more concrete character than a concept or its extension, as Frege thinks of it: a suitable aggregate can be photographed or travel by aeroplane.²⁰ Since he seems to nurture a degree of nominalist prejudice, this predisposes him in favour of the aggregate.

The obvious reply, from Frege's standpoint, is that it is the members of the aggregate, the objects falling under the concept, that are photographed; or, if the point is that they are photographed *together*, we are talking, not about an aggregate, but a system, whose cohesion requires that certain relations obtain between its components.²¹ Bell's answer is that this objection springs from the Fregean blindness to plural terms. Statements about aggregates are simply statements with plural subjects, and fall into two types: those that ascribe a distributive property, and those that ascribe a collective one.²² A distributive property holds good of the individual members of the aggregate, while a collective property does not. This is Frege's distinction between saying that the tree has green leaves, and saying that it has 1,000 leaves;²³ but Bell has a particular reason for holding that the ascription of a collective property relates to an aggregate, not to a concept. For a statement about an aggregate never makes 'ineliminable reference to a single thing called an aggregate', but is always a statement about several individual objects at once. The difference between predicating a distributive property and predicating a collective one is that the distributive property is itself ascribed to the members of the aggregate, whereas the collective property is not one that individual objects can possess; but a collective predication can still be analysed as a statement about the members of the aggregate, but as one ascribing some *other* property to them.

What is here meant by 'a statement *about* the members'? From the examples given by Bell, it is apparent that, in such a statement, the predicate expressing membership of the aggregate appears only as applying to individual members,

¹⁹ *Philosophie der Arithmetik*, p. 185; emphasis Husserl's. On pp. 188–9 he argues that, in Frege's example, the number four attaches, not to the *concept* 'horse that draws the Kaiser's carriage', but to its *extension*. It is possible only with the hindsight afforded by the writings of Frege's middle period to perceive clearly what is wrong with this objection, namely that Frege was not, like Husserl, an intensional logician, and that his notion of a concept is already an extensional one; but Husserl is also mistaken, at least on Frege's view, in equating, as he does, the notion of the extension of a concept with his own notion of an aggregate.

²⁰ Bell, *op. cit.*, p. 65.

²¹ See note 8.

²² This citation from Bell's book, and those immediately following, are from pp. 68–9.

²³ *Grundlagen*, § 22.

never with a bound variable in its argument-place. It is, however, evident that a transformation of the original statement about the aggregate into one that is, by this criterion, about its members will be possible only when we know which individual members the aggregate has; but, since this knowledge is not part of the content of the original statement, such a transformation, even when possible, is not an analysis.

This is very clear from Bell's attempt to make out his claim that Husserl's notion of an aggregate is a better tool for analysing the concept of number than Frege's notion of an aggregate. He proposes to explain such an ascription of number as 'There are at least three coins in the fountain' as meaning ' $F(a) \& F(b) \& F(c) \& a \neq b \& b \neq c \& a \neq c$ ', where, he says, '“F” designates the property of being a coin in the fountain, and “a”, “b” and “c” are arbitrary names'. He proclaims this analysis to be superior to Frege's, on the ground that it involves 'only objects and their properties', and does not invoke abstract entities like concepts. It is obscure why the property of being a coin in the fountain should be regarded as less abstract than the concept *coin in the fountain*. Bell appears temporarily to have forgotten that the use of 'concept' in Frege's terminology is only the consequence of his step-by-step adaptation of that of Kant, that the two-argument analogue of a concept is a relation, and that Frege equated the concepts under which an object falls with its properties.²⁴ In any case the analysis collapses when we ask what 'a', 'b' and 'c' name if there happen not to be any coins in the fountain. Plainly, the analysis results from ignoring the distinction between *saying* that there are at least three coins in the fountain and *seeing* that there are. It is precisely because Husserl was preoccupied with cases in which we have all the individual members of an aggregate in view, or at least in mind, that, in struggling to understand his notion of an aggregate, one is in danger of losing one's grip on that otherwise evident distinction. Bell, in endeavouring to vindicate the notion, comes close to assimilating it to the Fregean notion of a concept; but, in trying to preserve some difference between them, he finally unfits it for the work of being that to which a cardinality is attributed in any ascription of number. As employed by Husserl in the *Philosophie der Arithmetik*, the notion of an aggregate is simply incoherent, and remains so even after being explained by Bell. Husserl did not give us an alternative to Frege's account of ascriptions of number, but only an irremediable confusion.

²⁴ See 'Über Begriff und Gegenstand', p. 201.

CHAPTER 9

Two Strategies of Analysis

A false start

Rejecting formalism, Frege acknowledges that mathematical propositions have a content, rendering them true or false; his aim in *Grundlagen* is to make explicit the content both of ascriptions of number and of the propositions encountered in number theory. Number-words occur in two forms: as adjectives, as in ascriptions of number, and as nouns, as in most number-theoretic propositions. When they function as nouns, they are singular terms, not admitting a plural; Frege tacitly assumes that any sentence in which they occur as adjectives may be transformed either into an ascription of number – a sentence beginning with ‘There are’, followed by a number-adjective – or into a more complex sentence containing an ascription of number as a constituent part. Plainly, any analysis must display the connection between these two uses: it would not do to give separate explanations of number-adjectives and of numerals functioning as terms, without providing for any explicit relation between them. Otherwise, we should be unable to appeal to the equation ‘ $5 + 2 + 0 = 7$ ’ to justify inferring that there were seven animals in the field from the fact that there were five sheep, two cows and no other animals there. Evidently, there are two alternative strategies. We may first explain the adjectival use of number-words, and then explain the corresponding numerical terms by reference to it: this we may call the *adjectival* strategy. Or, conversely, we may explain the use of numerals as singular terms, and then explain the corresponding number-adjectives by reference to it; this we may call the *substantival* strategy. A radical version of the adjectival strategy would be to refuse to take numerical terms at face-value. According to this strategy, equations and other arithmetical statements in which numerals apparently figure as singular terms are to be explained, not merely *in terms of* adjectival uses of number-words, but by *transforming* them into sentences in which number-words occur only adjectivally. On this view, numerals are only spurious singular terms, when they apparently function as such: equations and other number-theoretic sentences have a grammatical structure that belies their true logical

structure, which is revealed only when they have been transformed into versions containing only number-adjectives. The analogously radical version of the substantival strategy is, on the face of it, less attractive. According to it, it is ascriptions of number and other sentences in which number-words occur adjectivally that have a misleading surface form: they contain hidden references to the objects denoted by numerical singular terms.

In § 55, Frege essays the adjectival strategy, which, as he says, suggests itself very naturally. That is to say, he experiments with three definitions of expressions involving the adjectival use of number-words, stated without appeal to any prior definitions of numerical notions, and, in particular, without appealing to the use of numerical singular terms. In stating them, he makes heavy use of his jargon. Instead of saying, 'There is just 1 *F*', or 'There is just 1 object which is *F*', or even 'There is just 1 object falling under the concept *F*', he says, 'The number 1 belongs to the concept *F*'. This, of course, obscures the fact that these are *adjectival* uses of number-words that he is defining; for, in his jargon, the number-words precisely appear as singular terms – 'the number 1', 'the number 0', 'the number (*n* + 1)' and so forth. When we strip the terminology of the jargon, writing 'There are just *n* *F*s' for 'The number *n* belongs to the concept *F*', and '*a* is an *F*' for '*a* falls under the concept *F*', the definitions assume the following forms:

'There are 0 *F*s' is to mean that:
for all *x*, *x* is not an *F*;

'There is just 1 *F*' is to mean that:
it is not the case that, for all *x*, *x* is not an *F*, and, for all *x* and *y*, if *x* is an *F* and *y* is an *F*, then *x* = *y*;

'There are just (*n* + 1) *F*s' is to mean that:
for some *x*, *x* is an *F*, and there are just *n* objects distinct from *x* which are *F*s.

If we use the notation ' $\exists_n x$ ' to mean 'there are just *n* *x*'s such that', and similarly for other indices, we may write these as follows:

' $\exists_0 x Fx$ ' means that $\forall x \neg Fx$;

' $\exists_1 x Fx$ ' means that $\neg \forall x \neg Fx \ \& \ \forall x \forall y (Fx \ \& \ Fy \rightarrow x = y)$;

' $\exists_{n-1} x Fx$ ' means that $\exists x [Fx \ \& \ \exists_n y (Fy \ \& \ x \neq y)]$.

Symbols like ' $\exists_0$ ' and ' $\exists_1$ ' are, of course, defined quantifiers, usually known in logical literature as 'numerically definite quantifiers'.

In § 56, Frege rejects these definitions, saying that they ‘suggest themselves so spontaneously in the light of our previous results that an explanation is needed why they cannot satisfy us’. He begins on the third definition, saying that it will be the first to arouse qualms. He agrees that, by appeal to this definition and the second one, it will be possible to explain the expression:

the number $1 + 1$ belongs to the concept F ,

and hence also the expression:

the number $1 + 1 + 1$ belongs to the concept F ,

and so on; that is, expressed without the jargon, to explain first ‘there are just $1 + 1$ F s’ and then ‘there are just $1 + 1 + 1$ F s’, and so on. That is to say, the third definition serves as a pattern for constructing further definitions, in the sequence ‘there are just 2’, ‘there are just 3’, and so forth: but Frege does not consider it to be in itself a proper definition. His objection to it is that ‘strictly speaking, the sense of the expression “the number n belongs to the concept G ”’ – that is, of the expression ‘there are just n G s’ – ‘is just as unknown to us as that of the expression “the number $(n + 1)$ belongs to the concept F ”’ – that is, of the expression ‘there are just $(n + 1)$ F s’. He states this objection very badly, in the notoriously inept words ‘we can never . . . decide by means of our definitions whether the number Julius Caesar belongs to a concept, whether that famous conqueror of Gaul is a number or not’. No one reading the book for the first time can have seen this remark as making much sense, let alone as relevant. Nevertheless, Frege’s objection to the third of the proposed definitions is perfectly sound.

He comes more to the point when he says:

It is only an illusion that we have defined 0 and 1; in reality we have only fixed the sense of the phrases

the number 0 belongs to
the number 1 belongs to;

but we are not licensed to pick out the 0 and the 1 in them as self-subsistent objects recognisable as the same again.

In §§ 56–61, Frege is attempting to establish that arithmetical statements containing numerals *must* be taken at face-value, and hence that the radical adjectival strategy is not feasible. Ontologically expressed, he is trying to establish that numbers must be regarded as objects. If the radical adjectival strategy could be made successful, they could not be so regarded: apparent singular terms for numbers would be spurious, and the only admissible uses

of number-words would be as adjectives, overt or in disguise. Instead of regarding (cardinal) numbers as objects, we should have to be concerned only with the corresponding second-level concepts denoted by expressions like 'there is just one', 'there are just three', 'there are denumerably many', and so on. It is for this reason that the passage from § 55 to § 61 is of such great importance: its purpose is to prove the crucial thesis that numbers are objects. It is not immediately apparent what the precise content of this thesis is: but it certainly involves that numerical terms have to be taken at face-value, and cannot be explained away. Hence Frege's anxiety to convict the definitions he proposed in § 55 of being logically amiss.

In the comment about picking out 0 and 1 as self-subsistent objects, however, he is assuming what he is trying to prove: a valid objection to the third of the proposed definitions is entangled with a *petitio principii*. Anyone with his wits about him, who sees nothing wrong with the definitions of § 55, will reply that he was not in the least concerned to pick out 0 and 1 as self-subsistent objects, that he knows that the phrases 'the number 0 belongs to' and 'the number 1 belongs to' – better, 'there are 0' and 'there is just 1' – have been defined only as a whole, and that there is no licence to extract the numerals '0' and '1' from them and treat them as having a sense independently of the wholes of which they are part: and he will add that he was not intending to do so. Frege himself views number-adjectives as essentially occurring only in the context 'there are just ...', since he believes that to explain ascriptions of number is thereby to explain all adjectival uses of number-words. Hence, if '0' and '1' are to be given independent senses for use in other contexts, those can only be contexts in which they function grammatically as singular terms, at least in Frege's eyes. Now no one pursuing a radical adjectival strategy will wish to recognise any such context as genuine; and so he will be unmoved by Frege's denial to him of the right to envisage '0' and '1' as having independent senses. Someone pursuing an adjectival strategy with a less ambitious goal will not hope to eliminate numerical singular terms, but only to explain them: but he thinks the way to do this is by first explaining the corresponding numerically definite quantifiers, and then to explain the numerical terms by appeal to them. He, too, will be unmoved by Frege's protest: he does not want to remove '0' and '1' from the contexts in which alone they have so far been explained: he merely wants to work up to explaining them for other contexts.

Frege believed that, in §§ 56 and 57, he had demonstrated the definitions proposed in § 55 to be logically faulty; in fact, however, he had pointed out a defect only in the third of the three definitions. The first and second definitions do not require anyone to pick out the numerals '0' and '1' from within the context: but the third definition does precisely that, by replacing them with a variable. When the phrases have been defined, as Frege rightly says, only as wholes, we can attach a sense to each only as a whole: we are therefore

certainly not entitled to replace a *part* of each by a variable, any more than we can replace all but the common parts of the words 'cowl', 'cowrie', 'coward' and 'cowed' by a variable. The point is very clearly put by Frege, in a similar connection, in the Appendix concerning Russell's paradox which he added to Volume II of *Grundgesetze*. In the body of the book, he has of course defined cardinal numbers as classes; here he is discussing the possibility of 'regarding class-names as pseudo-proper names, which would therefore in fact have no reference'. His comment on this proposal is:

They would then have to be regarded as parts of symbols that would have a reference only as a whole. One may indeed consider it advantageous, for some purpose or other, to construct different symbols so that certain parts of them coincide, without thereby making them into complex ones. The simplicity of a symbol demands only that the parts that one may distinguish within them do not have a reference on their own. Even what we are accustomed to construe as a numeral would in such a case not really be a symbol at all, but only an inseparable part of a symbol. A definition of the symbol '2' would thus be impossible; one would have instead to define several signs that contained '2' as an inseparable constituent, but were not to be thought of as composed of '2' and some other part. It would then be illicit to let such an inseparable part be replaced by a letter [by this, Frege means a variable]; for as regards the content, there would be no complexity. The generality of arithmetical propositions would thereby be lost.

In just the same way, the expressions 'there are just two', 'there are just three', and so on, contain a common part, as do the symbols ' $\exists_2$ ', ' $\exists_3$ ' and the rest: but, when each has been defined only as a whole, their possession of a common part is logically without significance, and we cannot retain that common part, while replacing the remaining constituent by a variable.

This is a cogent objection to the third definition as formulated in *Grundlagen*, § 55: but it presents no obstacle to the general strategy. The remedy lies to hand: the variable must stand not in place of a part of one of the defined symbols, but in place of the whole symbol. Each such symbol stands for a concept of second level, and so the variable will range over second-level concepts: we have to define an operator which transforms one quantifier (expression for a second-level concept) into another, and which may therefore be applied, not merely to the numerically definite quantifiers, but to all quantifiers. This is difficult to express in words, but easy to write symbolically. If we use the symbol '+' to represent our operator, the emended definition will take the form:

' $M^+_x Fx$ ' is to mean that $\exists x [Fx \ \& \ M_y (Fy \ \& \ x \neq y)]$.

The outcome of this will be that ' $\exists_1^+_x Fx$ ' has the force of 'There are just 2 Fs', ' $\exists_2^+_x Fx$ ' that of 'There are just 3 Fs', and so on, just as desired, while ' $\exists^-_x Fx$ ' will mean 'There are at least 2 Fs', ' $\exists^{++}_x Fx$ ' will mean 'There are at

least 3 F s', and so on. Again, if we take ' $Hx Fx$ ' as an abbreviation for ' $\forall x (x \text{ is a man} \rightarrow Fx)$ ', then ' $Hx \Phi x$ ' will stand for that concept of second level under which fall all first-level concepts under which all men fall. The formula ' $H^+ x Fx$ ' will then say that all men fall under the concept F , together with at least one other object that is not a man. As for ' $\forall^- x Fx$ ', it says that there fall under the concept F all the objects there are, and one more besides, and thus cannot ever be true; but this is no objection to our definition of the operator '+'.

In point of fact, however, even that definition is unsatisfactory from the standpoint of good notation, and, for the same reason, conflicts with the syntactic and semantic principles of *Grundgesetze*. As for notation, '+' suffers from being applicable only to a single quantifier-symbol like ' $\exists$ ' or ' $\forall$ ', whereas there also exist what may be called complex quantifiers, in the broad sense of the term 'quantifier' relevant to present purposes; ' $\forall x (x \text{ is a man} \rightarrow \Phi x)$ ' was an example of such a complex quantifier. That is why, before applying '+', it was necessary to define a single symbol ' H ' as equivalent to the complex quantifier. Such a necessity points to a defect in notation.

The operator '+' transforms a quantifier into another quantifier: it therefore stands for a function from second-level concepts to second-level concepts. Yet, on Frege's principles, as elaborated in *Grundgesetze* and other works of the middle period, the only admissible functions are those which have objects or truth-values as values. The only admissible operators, under this principle, are those having sufficiently many argument-places that, when they are all filled, either a singular term or a complete sentence results. The reason is that an incomplete expression can be arrived at only by extracting it from a complete one. To comply with this doctrine, we need an operator with two argument-places, one admitting an expression for a second-level concept (a simple or complex quantifier), and the other admitting one for a first-level concept. Great notational cumbrousness results, however, from employing an operator of this type, while observing Frege's requirement that every expression shall appear only with its due argument-places, filled, if necessary, by bound variables. Our binary operator would then take the form:

$$S_{F,x} (\Xi_y Fy, \Phi x).$$

Here ' F ' and ' x ' represent the variables bound by ' S ', while ' Ξ ' and ' Φ ' represent its argument-places, for a quantifier and a first-level predicate respectively. We should then define:

$$S_{F,x} (M_y Fy, Gx) \longleftrightarrow \exists x [Gx \ \& \ M_y (y = x \ \& \ Gy)].$$

As a result,

$$S_{F,x} (\exists_1 y Fy, Gx)$$

would say that there were just two *G*s (i.e. two objects falling under *G*), and we could define:

$$\exists_2 x Gx \longleftrightarrow S_{F,x} (\exists_1 y Fy, Gx).$$

Likewise, we could write:

$$S_{F,x} [\forall y (y \text{ is a man} \rightarrow Fy), Gx]$$

to mean that all men, and at least one other object, fell under the concept *G*: the notation would allow us to avoid having first to define the special quantifier ' $\exists_1 x \Phi x$ '.

In fact, there is an easier way to go about it. Instead of defining a successor operation, represented by '+' or 'S', we may define the binary quantifier 'The *F*s consist of the *G*s and one other object' (i.e. 'Everything falling under *G* falls under *F*, and just one object falls under *F* but not under *G*'). This, like 'Most *F*s are *G*s' and 'There are just as many *F*s as *G*s', stands for a relation between first-level concepts. If we write it as ' $D_x (\Phi x, \Psi x)$ ', we may readily define it thus:

$$D_x (Fx, Gx) \longleftrightarrow \exists x [\neg Gx \ \& \ \forall y (Fy \longleftrightarrow Gy \vee y = x)].$$

We may then define ' $\exists_2 x \Phi x$ ' by:

$$\exists_2 Fx \longleftrightarrow \exists G [\exists_1 x Gx \ \& \ D_x (Fx, Gx)].$$

The definition is of second order, requiring quantification over first-level concepts; but Frege had, of course, no objection to that.

Frege's sleight of hand

The upshot of all this is that Frege was right to object to the proposed definition of 'There are just $(n + 1) \dots$ ', but had no case against those of 'There are $0 \dots$ ' and 'There is just $1 \dots$ '; and, moreover, that the third definition, though unsound, can be corrected without affecting its essential spirit. Frege aimed, however, at much more than an emendation of one defective definition: he aimed to show all three definitions erroneous, and thereby to prove a purely adjectival strategy unfeasible, because numbers have to be recognised as being objects. In this, he utterly failed: in fact, § 56 may be stigmatised as the weakest in the whole of *Grundlagen*. The arguments lack all cogency: they more resemble sleight of hand. This is not to suggest that

they were deliberately designed to take in the reader: rather, Frege, impelled by his desire to establish that numbers are objects, seems to have been taken in by his own jargon. When 'There are 0 *F*s' and 'There is just 1 *F*' are expressed as 'The number 0 belongs to the concept *F*' and 'The number 1 belongs to the concept *F*', it looks more plausible to complain that the definitions do not entitle us to pick out 0 and 1 as self-subsistent objects; without the jargon, it would have been apparent that they were not meant to and did not need to.

The same holds good for the complaint that

We cannot prove, by means of the putative definitions, that, if the number *a* belongs to the concept *F* and the number *b* also belongs to it, then necessarily *a* = *b*. There would then be no way to justify the expression '*the* number which belongs to the concept *F*', and it would be altogether impossible to prove a numerical equation, since we should not be able to get hold of any determinate number.

Identity, as Frege understood it, is a relation between objects: the complaint therefore assumes what has yet to be proved, that numbers *are* objects. If they are to be so regarded, then indeed we shall have to be able to express the relation holding between a number *n* and a concept *F* when there are just *n* objects falling under *F*, and to prove that, for any concept *F*, there is only one such number; and certainly the proposed definitions do not suffice for this. But Frege has supplied no ghost of an argument for supposing that they cannot be suitably supplemented.

It is possible to interpret Frege's complaint in another way, not directly involving reference to numbers as objects. According to the principle stated subsequently by Frege, the analogue, for first-level concepts *F* and *G*, of the relation of identity between objects is co-extensiveness, as expressed by:

$$\forall x (Fx \longleftrightarrow Gx).$$

Presumably the analogue will hold for second-level concepts. For example, to express that what corresponds to identity holds between the second-level concept denoted by 'There is one more than 0 Φ ' and that denoted by 'There is just 1 Φ ', we may write:

$$\forall F (\exists_0^+ x Fx \longleftrightarrow \exists_1 x Fx);$$

and there would be no difficulty in proving this. Such a proposition would be the analogue, for number-words used adjectivally, of the equation

$$0' = 1,$$

where the prime symbol denotes the successor operation. When we wished, however, to state the analogue of the proposition

$$n \text{ is the number of } Fs \ \& \ m \text{ is the number of } Fs \rightarrow n = m,$$

we should need to take a little care. Where 'N' and 'M' are free variables for quantifiers, ranging over second-level concepts, we cannot simply write:

$$(1) \quad N_x Fx \ \& \ M_x Fx \rightarrow \forall G (N_x Gx \longleftrightarrow M_x Gx),$$

since it obviously is not true: there are at least nine planets, and there are fewer than a hundred planets, but the second-level concepts denoted by 'there are at least nine' and by 'there are fewer than a hundred' are far from being co-extensive. We need a means to restrict the generalisation to those second-level concepts which correspond to cardinal numbers, namely those under which fall precisely those first-level concepts having some specific cardinality. But Frege offers not the slightest reason for thinking that this cannot be done; in fact, no obstacle whatever stands in the way of doing it. Obviously, it will be possible only if we have available the notion of cardinal equivalence, which Frege later defines in terms of one-one mappings, expressed in natural language by sentences of the form 'There are just as many *F*s as *G*s'; we may write it symbolically as the binary quantifier ' $\approx_x (\Phi_x, \Psi_x)$ '. A quantifier ' $M_x \Phi_x$ ' then serves to answer the question 'How many?' if the following holds good generally:

$$(2) \quad M_x Fx \ \& \ \approx_x (Fx, Gx) \rightarrow M_x Gx.$$

Formula (2) will hold good of quantifiers expressing such notions as 'there are less than a hundred . . .', 'There are infinitely many . . .', 'There is an odd number of . . .' and so forth; the more important condition is that the quantifier should serve to give a *definite* answer to the question 'How many?', which it will do if the stronger condition:

$$(3) \quad M_x Fx \rightarrow [\approx_x (Fx, Gx) \longleftrightarrow M_x Gx]$$

holds generally. Formula 1 may then be proved to hold for all quantifiers ' $M_x \Phi_x$ ' and ' $N_x \Phi_x$ ' that satisfy (3) for all *F* and *G*.

Admittedly, Frege's complaint relates, not to the definitions in their amended form (that is, with the amended version of the third one), but to the original definitions as formulated in § 55; the idea of employing general variables for second-level concepts has therefore not been introduced. But, even so, were the third definition not objectionable on other grounds, there would be reason to introduce such variables, and no obstacle to doing so; and, by their means,

it would be possible to devise a way of restricting the quantifiers considered in the formula (1) to those in the sequence

$$' \exists_0 x \Phi x', ' \exists_1 x \Phi x', ' \exists_2 x \Phi x', \dots,$$

that is to say, to those assigning a natural number as the cardinality of a concept. In fact, given Frege's subsequent definition of 'finite number', it would be easy: all that would be needed would be the analogue of that definition in the adjectival mode. That, indeed, would involve quantification over third-level concepts. This would be inexpressible in natural language, and the notation would thereby become exceedingly cumbersome – given always the requirement that all argument-places be explicitly visible; but there would be no conceptual difficulty. The reader to whom the topic is new cannot be expected to perceive this. He may well be persuaded by Frege that there is a difficulty in principle, simply because he himself cannot see the way out of a merely apparent difficulty. This, together with the confusing effect of his jargon, is why Frege's argument in §§ 56–7 has the character of sleight of hand.

Apart from the valid point about the third definition, Frege's arguments in § 56, however charitably interpreted, at most point out that the proposed definitions do not accomplish all that we need, and that massive supplementation will be necessary; but this should in any case be obvious. Whether the supplementation is conceived as involving the substantival use of numerical terms, or as shunning it in favour of an exclusively adjectival use, Frege says nothing to show that we could not build upon the base provided by the first two proposed definitions and an emended version of the third.

How Frege handles number-adjectives

In § 57 Frege explains that 'when, in the proposition "the number 0 belongs to the concept *F*"', we consider the concept *F* as the real subject, 0 is only an element in the predicate'. For this reason, he says,

I have avoided calling a number like 0, 1 or 2 a *property* of a concept. The individual number, as being a self-subsistent object, appears precisely as a mere part of the predicate.

This, of course, merely continues the prevarication of § 56. Considered as an object, a number cannot, indeed, be a property of a concept: no object can be identified with a second-level concept. But Frege has still not proved that numbers are to be regarded as objects. The expression for the second-level concept indeed contains a number-word, but, when phrased in a natural way, only in the form of an adjective. That adjective is admittedly only an inseparable

part of the second-level predicate; but no argument has yet been given why we should want to separate it, let alone construe it as a numerical *term*.

Frege continues by urging that, in number-theoretic statements and equations, number-words occur, for the most part, in substantival form, as singular terms; we have therefore 'to regard the concept of number in such a way that it can be used in science'. He then has to answer the question how, if we are to treat number-words as 'proper names', we can construe their use as adjectives. He replies that this is easily done. 'The sentence "Jupiter has four moons"', for example, can be converted into "The number of Jupiter's moons is four"', where the word 'four' functions as a proper name of the number 4; the transformed sentence has the form of an identity-statement. Frege's solution of the problem of relating the adjectival and substantival uses of number-words to one another is thus, apparently, to treat the adjectival uses as disguised forms of the substantival ones. A sentence like 'Jupiter has four moons' or 'There are four moons of Jupiter' does not appear, from its surface form, to contain any reference to the number four, regarded as an abstract (non-actual) object. Nevertheless, according to the analysis Frege here suggests, its surface form is misleading: when its deep structure is uncovered, it can be recognised as really being a statement of identity between the object denoted by 'the number of Jupiter's moons' and that denoted by 'the number 4'.

The contention has a high degree of implausibility. Worse, its acceptance undermines Frege's appeal to the surface forms of number-theoretic sentences. If it is legitimate for analysis so to violate surface appearance as to find in sentences containing a number-adjective a disguised reference to a number considered as an object, it would necessarily be equally legitimate, if it were possible, to construe number-theoretic sentences as only appearing to contain singular terms for numbers, but as representable, under a correct analysis of their hidden underlying structure, by sentences in which number-words occurred only adjectivally. The possibility is that aimed at by the radical adjectival strategy, which, for all his rhetoric, Frege has done nothing, in §§ 56–7, to prove unfeasible. If the appeal to surface form, in sentences of natural language, is not decisive, then it cannot be decisive, either, when applied to sentences of number theory. Frege has merely expressed a preference for the substantival strategy, and indicated a means of carrying it out: he has in no way shown the adjectival strategy impossible, as he is purporting to have done.

Some defensive moves

In the remainder of this passage, §§ 58–61, Frege does no more than defend the thesis that numbers are objects against objections, reiterating that an object may be objective but yet not actual, nor, in particular, spatial, and observing

that even actual objects may not be capable of being imagined, and so, in his sense of the word 'idea', may have no idea associated with them, or, at best, an irrelevant or manifestly inadequate one. The impossibility of forming an idea of its content is no ground for denying meaning to a word, for meaning is not constituted by ideas or mental images. At this place (§ 60), Frege invokes the context principle, stated in the Introduction.

We must always keep a complete sentence before our eyes. Only in it do the words really have a meaning. The inner images which may hover before us do not necessarily correspond to the logical constituents of the judgement. It is enough if the sentence as a whole has a sense; it is through this that the parts obtain their content also.

Whatever may be thought of the context principle thus strongly enunciated, Frege's general contentions concerning objects, in §§ 58–61, are evidently quite sound. They successfully defend the thesis that numbers are objects against fallacious objections; but they do nothing to establish that thesis, so crucial for Frege's philosophy of arithmetic, and it remains, at the end of § 61, wholly devoid of cogent justification.

CHAPTER 10

Frege's Strategy

The linguistic turn

The highly unsatisfactory passage from § 55 to § 61 of *Grundlagen* is followed by the most brilliant and philosophically fruitful in the book, and the most important for Frege's philosophy of mathematics, and, indeed, his philosophy generally. It extends from § 62 to § 69, and is highly significant, not merely for the understanding of Frege's own philosophy of mathematics, but for the philosophy of mathematics in general.

Having made, in § 55, what, in §§ 56–7, he then rejected as a false start, Frege now adopts a fresh strategy. Of these inspired sections, § 62 is arguably the most pregnant philosophical paragraph ever written. It does not merely introduce the important notion of a criterion of identity, considered as associated with any proper name or other singular term: it is the very first example of what has become known as the 'linguistic turn' in philosophy. Frege's *Grundlagen* may justly be called the first work of analytical philosophy.

After § 61, Frege assumes that he has shown that numbers are objects, and must be treated as such. Since they are objects, he begins his new enquiry by posing the Kantian question, 'How are numbers given to us?'. Kant's doctrine was, of course, that objects can be given only through sensible intuition. Frege has, however, already rejected the notion that number is any kind of perceptible feature of things, or that numbers are objects of which we can have intuitions. The problem is therefore an acute one, particularly for anyone influenced by Kant, as few philosophers were not at the time when Frege was writing.

His solution was to invoke the context principle: only in the context of a sentence does a word have meaning. On the strength of this, Frege converts the problem into an enquiry how the senses of sentences containing terms for numbers are to be fixed. *There* is the linguistic turn. The context principle is stated as an explicitly linguistic one, a principle concerning the meanings of words and their occurrence in sentences; and so an epistemological problem, with ontological overtones, is by its means converted into one about the meanings of sentences. The context principle could have been given a non-

linguistic formulation. It would then have said that we do not conceive of objects save as ingredients in states of affairs, or that we cannot apprehend an object save in the course of recognising something as holding good of it. But Frege gave it, from the outset, a linguistic formulation; and so, when he comes to invoke it, he makes the linguistic turn. He offers no justification for making it, considers no objection to it and essays no defence of it: he simply executes the manoeuvre as if there were no novelty to it, and does it so skilfully that the reader scarcely perceives the novelty. Yet it was in fact unprecedented in the history of philosophy. Plenty of philosophers – Aristotle, for example – had asked linguistic questions, and returned linguistic answers: Frege was the first to ask a *non*-linguistic question and return a linguistic answer. If it were on the strength of *Grundlagen*, § 62 and its sequel alone, he would still deserve to be rated the grandfather of analytical philosophy.

Criteria of identity

The principle of criteria of identity enunciated in § 62 states that:

If the symbol *a* is to designate an object for us, we must have a criterion that will in every case decide whether *b* is the same as *a*, even if it is not always within our power to apply this criterion.

Accordingly, numerical terms must be provided with a criterion of identity; and this means that there must be a determinate, non-circular condition for the truth of an identity-statement connecting them. Frege does not at this point discuss the meanings of numerical equations, however. Instead, he tacitly assumes that the fundamental type of terms standing for numbers consists of those of the form ‘the number of *F*s’, or, in his jargon, ‘the number belonging to the concept *F*’, without attempting to justify this choice. The choice is, after all, natural, given the demand for a criterion of identity. If we were asked for a criterion of identity for distances, we should not explain how to judge the truth of statements of the form ‘1 metre = 39.37 inches’: we should explain what determined whether the distance between *P* and *Q* was the same as that between *R* and *S*. So the question becomes how to specify the condition for the truth of a statement of the form ‘The number of *F*s is the same as the number of *G*s’.

The sequence of thought leading up to this question makes it utterly unnatural to reply at this point that, since we already know the meaning of ‘is the same as’, what is needed is to specify the meaning of a term of the form ‘the number of *F*s’. As a matter of fact, that is precisely what, in § 68, Frege eventually does; and he himself insists, in § 63, that we do already know the meaning of ‘is the same as’, namely as given by Leibniz’s law:

$$x = y \longleftrightarrow \forall F (Fx \longleftrightarrow Fy),$$

as he explains in § 65, where he speaks of the law as a 'definition' of identity. In his middle period, he rated identity indefinable, on the ground that every definition must take the form of a identity-statement, generalised or otherwise. Nevertheless, the reply that, since we know what the sign of identity means, we have to define the operator 'the number of Φ s', would at this stage be quite unnatural on the part of anyone who has gone along with Frege so far. The doctrine of criteria of identity involves that we shall explain terms of the form 'the number of F s' by explaining when two such terms denote the same number, or, in other words, when an identity-statement connecting them is true. If so, it would hardly be reasonable to propose explaining that by first defining the operator, 'the number of Φ s', which we may call the 'cardinality operator': for that would appear to render the doctrine wholly nugatory. Rather, we have to lay down the truth-conditions of statements of the form

- (1) the number of F s = the number of G s

in some non-question-begging way.

The notion of a criterion of identity, which Frege introduced into philosophy, has been widely employed by analytical philosophers in recent decades; but it is far from simple to explain, and Frege himself, who never mentioned it again after *Grundlagen*, provided little help. A criterion of identity for C s ought not in general to be equated with a necessary and sufficient condition for the truth of identity-statements connecting terms for C s, although it frequently is. Such an interpretation would lead us to say that the criterion of identity for countries is that they should have the same boundaries: for the truth of 'Iran and Persia are the same country' stands or falls with that of 'Iran and Persia have the same boundaries'. But this is a criterion we can apply only when we already know what 'Iran' and 'Persia' denote: it will not help us to decide whether Edinburgh and Birmingham, or Kiev and Moscow, are or are not in the same country. The same mistake is, I think, involved in Davidson's claim that having the same causes and the same effects is the criterion of identity for events,¹ and, far more disastrously, in Ayers's claim that spatio-temporal continuity is the criterion of identity for material objects:² we cannot know the causes and effects of an event until we know what that event comprises, and we cannot trace the path of an object through space unless we are already able to identify that object from one moment to another. On the contrary, the criterion of identity for objects of a given sort is something we

¹ Donald Davidson, in his 'The Individuation of Events', originally published in N. Rescher (ed.), *Essays in Honor of Carl G. Hempel*, Dordrecht, 1969, reprinted in D. Davidson, *Essays on Action and Events*, Oxford, 1980, pp. 163–80, says, 'We have not yet found a clearly acceptable criterion for the identity of events. Does one exist? I believe it does, and it is this: events are identical if and only if they have exactly the same causes and effects' (p. 179).

² Michael Ayers, 'Individuals without Sortals', *Canadian Journal of Philosophy*, vol. IV, 1974, pp. 113–48.

have to learn before we are in a position to know what a term for an object of that sort denotes. It must therefore be thought of as determining the condition for the truth of identity-statements connecting terms whose use has not yet been completely fixed; we know that they are meant to function as singular terms, and we know something of when they can be used and how the truth of certain statements involving them is determined, but the criterion of identity must be supplied if we are to be able to use them as full-fledged terms.

An alteration of course

The impetus of the discussion pushes us to take a further step. We are aiming at arriving at an explanation of the cardinality operator via a specification of the truth-conditions of a statement of the form (1). The cardinality operator has been tacitly accepted as the fundamental operator forming terms for numbers. It follows, therefore, that, to avoid circularity, our specification of truth-conditions should appeal only to expressions not involving numerical terms, viz. singular terms denoting numbers presented as objects. That is precisely the kind of specification that Frege gives. A sentence of the form (1) is to be specified to be equivalent to the corresponding sentence 'There are just as many *F*s as *G*s', or, in Frege's jargon, 'The concept *F* is equinumerous to the concept *G*'. At the outset, indeed, in § 63, Frege makes this appear so innocuous a step that he conflates it with the definition of 'There are just as many *F*s as *G*s' in terms of one-one mapping to mean 'There is a one-one map of the *F*s on to the *G*s'. We should, however, keep the two steps distinct. The first question is whether 'There are just as many *F*s as *G*s' should be explained as meaning 'The number of *F*s is the same as the number of *G*s', or, conversely, 'The number of *F*s is the same as the number of *G*s' explained as meaning 'There are just as many *F*s as *G*s'. Frege decides in favour of the latter direction of explanation in § 65. More precisely, he decides in favour of explaining 'The direction of the line *a* is the same as the direction of the line *b*' as meaning 'The line *a* is parallel to the line *b*' rather than conversely, adding that the discussion can in essentials be transferred to the case of the identity of numbers. The proposed explanation would obviously be fruitless if it were not then possible to give a definition of 'is parallel to' without appealing to the notion of a direction: but Frege does not trouble himself to discuss how such a definition should be framed. The two steps are distinct, even though the first would be useless if the second were impossible. Likewise, it would be useless to explain 'The number of *F*s is the same as the number of *G*s' as meaning 'There are just as many *F*s as *G*s' if it were not then possible to define the latter by means not involving terms for numbers as objects; but, for all that, the two steps are distinct.

Frege succeeds in making all this seem entirely natural, and all but inescap-

able; and yet it is only his skill in steering his readers in just the direction in which he wants them to go that prevents us from being amazed at the step he is taking. The whole drift of the argument in §§ 55–61 had appeared to be to reject, not merely the radical adjectival strategy, but an adjectival strategy of any kind. Yet the form of sentence 'There are just as many *F*s as *G*s' must clearly be placed on the adjectival rather than the substantival side of the divide. It contains no number-adjective, indeed; but, just as a sentence of the form, 'There are four *F*s', assigns a property to a first-level concept, so one of the form, 'There are just as many *F*s as *G*s', states a relation between two first-level concepts, that of equinumerosity: unlike a sentence of the form, 'The number of *F*s is the same as the number of *G*s', it involves no reference to or quantification over numbers treated as objects. It is far from apparent why, if a sentence like 'Jupiter has four moons' is to be explained as meaning 'The number of Jupiter's moons is 4', one like 'Jupiter has just as many moons as there are figures in the syllogism' should not be explained as meaning 'The number of Jupiter's moons is the same as the number of figures of the syllogism'. Yet, having proposed the first explanation in § 57, Frege here vehemently rejects the latter. He indeed insists on the fundamental status of the equivalence:

$$(2) \quad \approx, (Fx, Gx) \longleftrightarrow \text{card}, [Fx] = \text{card}, [Gx],$$

where 'card, [*Fx*]' symbolises 'the number of *F*s'. But he argues emphatically that the direction of explanation must be from left to right, from the adjectival to the substantival form. His is therefore a mixed strategy, neither purely adjectival nor purely substantival.

Numbers and directions

As already observed, Frege does not argue directly that the direction of explanation in (2) must be from left to right, but argues instead for the priority of 'The line *a* is parallel to the line *b*' over 'The direction of *a* is the same as the direction of *b*'. In § 64 he adopts the expository device of switching the discussion from the question with which he is actually concerned, namely by what means the second-level operator 'the number of *Φ*s' is to be introduced and explained, to the discussion of what he trusts will be perceived as an analogous case, namely how to introduce and explain the first-level operator 'the direction of *ξ*'. He is explicit about his intentions in a footnote to § 65:

I here speak of parallelism in order to be able to express myself more conveniently and to be more easily understood. What is essential to these discussions will easily be able to be transferred to the case of identity of numbers.

He continues his whole discussion in terms of this analogue until he arrives

at the final conclusion almost at the very end, half-way through § 68; only at that point does he revert to the real topic, without more ado applying the conclusion he has reached concerning the correct means of introducing the direction-operator to that of introducing the cardinality operator. This procedure rests upon the assumption, which he expresses in the footnote, but makes no attempt to argue, that the two operators *are* analogous in the relevant respects.

In fact, they are not. The analogy fails on two counts. By far the more important will be scrutinised in detail later. This turns on the fact that the argument-place of the cardinality operator is to be filled by a concept-word, that of the direction-operator, on the other hand, by a singular term standing for a line; this discrepancy in level makes a significant difference to the upshot of the discussion. The second failure of the analogy lies in the fact that one operator belongs to arithmetic and the other to geometry. Frege was never tired of emphasising the difference in character and status between the two branches of mathematics: he might therefore be expected to have taken care to include nothing in his reasoning about the direction-operator that made appeal to its specifically geometrical content. He failed to take such care. He argues in § 64 for the conceptual priority of the relational expression ' ξ is parallel to ζ ' over the term-forming operator 'the direction of ξ '. The argument he gives is that we have intuitions of straight lines and an 'idea' of parallel lines, but no intuition of a direction, whereas everything geometrical must be intuitive in origin: hence the operator must be defined in terms of the relation of being parallel, rather than lines being defined to be parallel if their directions coincide.

According to Frege, geometry rests on intuition, while arithmetic does not. It follows that the argument for the conceptual priority of the notion of parallelism over that of a direction cannot be adapted, without being greatly modified, to a proof of the conceptual priority of the notion expressed by 'just as many' over that of a number. We could not argue that we have intuitions of concepts, and an 'idea' of their equinumerosity, but no intuitions of numbers. The argument needs to be thoroughly recast for this case; and Frege has overlooked the necessity for indicating how it should be recast.

The attempt has been made to defend him by maintaining that, contrary to his express declaration in the footnote, he did not intend to transfer the argument from the one case to the other.³ If he had not, then §§ 64–7 would represent only an irrelevant excursus into the foundations of geometry, and when, in the middle of § 68, he abruptly lurches back into a discussion of arithmetic, his pronouncement that the cardinality operator must be defined in a manner analogous to the direction-operator would have been an assertion for which he had given no argument whatever. Obviously, this is wrong. The

³ By Gregory Currie in his review of my *Frege: Philosophy of Language*, in the *British Journal for the Philosophy of Science*, vol. 27, 1976, at pp. 85–6.

evident, as well as the stated, purpose of the passage from § 64 to the middle of § 68 is to conduct a discussion directly relevant to the central problem, how the cardinality operator is to be introduced, by treating of an almost perfectly analogous case. The analogy was not perfect, and Frege's discussion gave inadequate recognition to its imperfection: but any other interpretation of this passage reduces it to nonsense.

Problems

One problem that arises from the passage extending from § 62 to § 69 is thus to scrutinise the supposed analogy between the concept of a direction and that of a number. How did Frege intend us to transpose the argument for the conceptual priority of the notion of parallelism over that of a direction into one for the conceptual priority of the notion expressed by 'just as many' over that of a number? Does the fact that the cardinality operator is of second order, while the direction-operator is of first order, make a significant difference to Frege's argument?

The passage as a whole is concerned to explore the possibility of introducing the cardinality operator by outright stipulation of the equivalence (2) between 'There are just as many *F*s as *G*s' and 'The number of *F*s is the same as the number of *G*s', assuming the former to have been antecedently explained in terms of one-one mappings. Much of the discussion is conducted in terms of the analogue, namely a means of introducing the direction-operator by stipulating the equivalence between 'The line *a* is parallel to the line *b*' and 'The direction of *a* is the same as the direction of *b*', assuming the former to be already understood. In §§ 63–5 such an explanation is defended against objections; but, at the last moment, Frege decides that there is one objection against which no defence can be provided. He therefore abandons the proposal in favour of an explicit definition of the direction-operator, and, by parity of reasoning, of the cardinality operator. This decision comes as a shock to the reader, because, in § 62, the passage has opened with the terse enunciation of two principles which seem to make the proposal, rejected in §§ 66–9, mandatory. The first was the context principle, offered as supplying *the* answer to the initial question how numbers are given to us; § 62 had opened as follows:

How, then, is a number to be given to us, if we can have no idea or intuition of it? Only in the context of a sentence do the words mean anything. It therefore becomes a matter of explaining the sense of a sentence in which a number-word occurs.

Only two sections previously, Frege had glossed the context principle by observing that 'it is enough that the sentence as a whole should have a sense;

it is through this that its parts obtain their content'. The principle thus appears to demand that we should not attempt to assign a meaning to terms for numbers on their own, independently of the particular forms of sentence in which they occur, but should fix the meanings of those terms by laying down the senses of such forms of sentence: it could hardly be foreseen that the upshot of the whole enquiry would be an outright definition of the cardinality operator, considered apart from any particular context.

The second principle enunciated in § 62 was that of criteria of identity; and this was presented as determining *which* sentences involving terms formed by means of the cardinality operator we need in the first instance to explain:

We have already settled that number-words are to be understood as standing for self-subsistent objects. There is thereby given to us a category of sentences which must have a sense, namely sentences which express recognition. If the symbol *a* is to designate an object for us, we must have a criterion which decides in all cases whether *b* is the same as *a*, even if it is not always within our power to apply this criterion. In our case we must explain the sense of the sentence

'The number belonging to the content *F* is the same as that which belongs to the concept *G*';

that is, we must render the content of this sentence in another way, without using the expression

'the number belonging to the concept *F*'.

We shall thereby provide a general criterion for the identity of numbers. When we have thus obtained a means of laying hold on a determinate number and recognising it as the same again, we can give it a number-word as its proper name.

This had seemed quite explicit: terms of the form 'the number of *F*s' are to be explained *by* laying down the sense of an identity-statement connecting two such terms, and hence not directly; for if they were explained directly, there would appear to be no question of laying down the sense of the identity-statement, which would, instead, have to be derived from that explanation. And yet, that is what in the end Frege does: he gives an explicit definition of the operator used to form such terms for numbers. What, then, was the point of stating the two principles, the context principle and the doctrine of criteria of identity, and doing so with such emphasis? A first thought might be that they served merely as permissive, not as prescriptive: they established that an explanation of the cardinality operator by stipulating the sense of an identity-statement involving it was not to be ruled out a priori, even though it eventually proved unfeasible for more particular reasons. This hypothesis, however, is quite incompatible with the great stress Frege lays upon the two principles, and particularly on the context principle, cited in the Introduction as one of

three methodological precepts that have governed the composition of the whole book, and cited again, with a flourish of trumpets, in § 60. Moreover, the interpretation would have it that the two principles were ones in which Frege indeed believed, but which turned out to be irrelevant to his project. If that were so, they would hardly figure again in his final summary of his argument (§§ 106–8); but the context principle is reiterated once more in § 106, as a fundamental principle without which it is impossible to avoid a physicalist interpretation of number without falling into a psychologistic one. Frege thus considers it to have been an essential aid to arriving at his conclusions. Moreover, in §§ 106–7, Frege lays the same stress as before on fixing the sense of an identity-statement (a 'judgement of recognition'). We thus have an acute exegetical problem to resolve. It is: what, then, *is* the purport of the context principle, and what that of the doctrine of criteria of identity, when something that appeared to be the consequence, and the intended consequence, of both is in the end repudiated?

The strategy in detail

These are by no means the deepest, let alone the only, problems raised by §§ 62–9. To discuss them adequately, however, we must have in view Frege's entire strategy for defining the basic arithmetical notions in logical terms and deriving from the laws of logic the fundamental principles of arithmetic as so interpreted. We may list the arithmetical notions dealt with in *Grundlagen* as follows, setting those which belong with the adjectival use of number-words on the left, and those which belong with their substantival use on the right. Those on the right involve either terms for numbers or individual variables ranging over them; those on the left contain no numerical terms, and we are not required to take the range of their individual variables to include numbers.

There are just as many <i>F</i> s as <i>G</i> s	the number of <i>F</i> s
	<i>n</i> is a number
There are 0 <i>F</i> s	the number 0
There is just 1 <i>F</i>	the number 1
There is one more <i>F</i> than there are <i>G</i> s	<i>n</i> is a successor of <i>m</i>
	$n > m$
	$n \geq m$
	<i>n</i> is a finite number

Frege begins his chain of definitions at the top of the left-hand column, by defining 'There are just as many *F*s as *G*s' to mean 'There is a one-one map of the *F*s on to the *G*s' (§§ 63 and 72); the notion of a one-one map is itself defined in §§ 70–2. He then crosses to the right-hand column; as stated above, he first suggests explaining 'the number of *F*s' by stipulating that 'The number

of Fs = the number of Gs ' is to be equivalent to 'There are just as many Fs as Gs ', but then rejects this idea, and defines 'the number of Fs ' explicitly to mean 'the class of concepts G such that there are just as many Fs as Gs ' (§§ 68 and 72): the extension of a second-level concept is a class of concepts, just as the extension of a first-level concept is a class of objects. Frege then defines the predicate ' n is a (cardinal) number' to mean 'for some F , n is the number of Fs ' (§ 72). The number 0 is defined to be the number of objects not identical with themselves (§ 74), and the number 1 as the number of objects identical with the number 0 (§ 77). The corresponding expressions on the left-hand side, 'There are 0 Fs ' and 'There is just 1 F ', have in fact been satisfactorily defined in § 55; but these definitions have been rejected in favour of explaining them instead in terms of their right-hand counterparts, that is, as meaning, respectively, 'The number of Fs = 0' and 'The number of Fs = 1'.

The relation expressed by ' n is a successor of m ' is defined in § 76 to mean 'For some F and some x , n is the number of Fs and m is the number of objects distinct from x that are F '. In this sense, a successor is not, in general, the next greatest cardinal number: each transfinite cardinal will be its own successor; until this relation has been proved to be many-one, the definite article before 'successor' remains unjustified. The binary quantifier expressed by 'There is one more F than there are Gs ' is not in fact defined by Frege. 'The Fs comprise the Gs and one other object' was suggested above as a suitable replacement for the defective definition of 'There are just $(n + 1)$ Fs ' suggested in § 55; from that, 'There is one more F than there are Gs ' can easily be defined with the help of 'There are just as many Fs as Gs '. Frege would, of course, define 'There is one more F than there are Gs ' to mean 'The number of Fs is a successor of the number of Gs '. The expression ' $n > m$ ' is not used in the above table in the sense ' n is a larger cardinal number than m ', but in place of Frege's locution ' n follows m in the series of natural numbers', which holds if n can be reached from m by a finite number of steps (at least one) going from a number to a successor of it: this notion is obtained by applying the *Begriffsschrift* definition of the proper ancestral to the successor relation, as is done in *Grundlagen*, §§ 79 and 81, and hence as meaning ' n falls under every concept hereditary with respect to the successor relation under which any successor of m falls': a concept is hereditary with respect to a relation if every object falls under it to which another object falling under it stands in that relation. The expression ' $n \geq m$ ' likewise represents Frege's locution ' n belongs to the series of natural numbers beginning with m ', defined in § 81 to mean ' $n > m$ or $n = m$ '. Finally, ' n is a finite number', which is Frege's terminology for ' n is a natural number', can simply be defined to mean ' $n \geq 0$ ' (§ 83). The effect of this definition is of course that n will be a finite (natural) number if it falls under every concept hereditary with respect to successor under which 0 falls; that is, if ordinary mathematical induction holds good for it. From this, the principle of induction, which constitutes the fifth Peano

axiom, is immediately derivable, while the first and second Peano axioms, stating that 0 is a natural number and that the successor of a natural number is again a natural number, are equally readily derived.

This, then, is Frege's mixed strategy of definition. The chain of definitions starts at the top of the left-hand side, moves across to the top of the right-hand side, and then proceeds down that side, with the left-hand expressions other than the top one defined in terms of the corresponding right-hand expressions. The strategy thus rests on three fundamental principles. The first, not stated explicitly, is that all notions on the right-hand, substantival, side are ultimately derivable from that of the cardinality operator: the most basic numerical terms are those of the form 'the number of *F*s', and all other arithmetical notions are to be defined by means of them. The second principle is not only stated by Frege, but vigorously argued for by him in §§ 62–9: it is that the adjectival (left-hand) notion of cardinal equivalence expressed by 'There are just as many *F*s as *G*s' is conceptually prior to the cardinality operator, which accordingly must be defined in terms of it: that is why the chain of definitions must *start* on the left-hand side. The third principle is stated by Frege in § 57, but only cursorily argued for. It is that all other adjectival notions must be defined in terms of their substantival (right-hand) analogues. The argument, such as it is, is that only so is it possible to establish the required logical connection between substantival and adjectival notions.

Essentially the same strategy is followed in *Grundgesetze*, Part II, save that there Frege does not bother to introduce particular symbols for 'is a number' and 'is a finite number', since he can express those notions quite tersely without them; but he calls attention in words to the means he has for expressing them.⁴ In the logical system of that book, the notion of a concept is generalised to that of a function with arbitrary objects as values; since the truth-values *true* and *false* are treated as objects, a concept can then be regarded as a function all of whose values are truth-values. Every first-level function has a value-range, belonging to the domain of objects; the abstraction operator, forming terms for value-ranges, is primitive, and a class is the value-range of a concept. In *Grundlagen*, appeal to the notion of a class is cut down to the very minimum: it is used for the definition of the cardinality operator and for nothing else. In *Grundgesetze*, however, it is used very freely, and this gives a somewhat different form to several of the definitions. Without introducing any additional primitive, it is possible for Frege to introduce the notion of a 'double value-range', or extension of a binary function $f(\xi, \zeta)$: this is the value-range of the unary function which maps any object a on to the value-range of the unary function $f(\xi, a)$. In the special case that the binary function is a relation (*Beziehung*), the double value-range thus obtained serves as the extension of that relation; Frege calls the extension of a relation a *Relation*. Since a function

⁴ Vol. I, § 42, p. 58, and § 46, p. 60.

is required to be defined for all possible arguments, however, Frege has to substitute, for the notion of a function with a restricted domain, that of a many-one relation; he had done just the same in *Begriffsschrift* and *Grundlagen*. He first defines this notion;⁵ more precisely, he defines the notion of being the extension of a many-one relation. He then defines the notion of a mapping;⁶ not, however, as in *Grundlagen*, as a one-one relation mapping the objects falling under one concept on to those falling under another, but as the extension of a one-one relation mapping the members of one class on to the members of another.

Having defined the (extension of the) converse of a relation,⁷ he then defines the cardinality operator.⁸ This is done in a similar way to that used in *Grundlagen*; the difference is that, in *Grundgesetze*, the operator is a first-level one, and a number is given as a class of classes rather than of concepts. The operator can be applied to an arbitrary object *a*: its value will be the class of classes that can be mapped one-one on to *a*, in other words, of classes having the same number of members as *a*; of course, if *a* is not a value-range, it will have no members. The *Grundlagen* notion of a class whose members are concepts is undoubtedly a bizarre one, adopted by Frege in that book in order not to use the notion of a class (of an extension of a concept) save where it became strictly necessary. In *Grundgesetze*, however, he did not want, and saw no need, to incorporate into the system a higher-level abstraction operator forming terms for the value-ranges of second-level functions; hence his definition of cardinal numbers in that book more closely resembles that which Russell was to give. Allowing for this difference in the definition of the cardinality operator, the *Grundgesetze* definition of the (extension of the) successor relation⁹ is just as in *Grundlagen*; the same applies to the definitions of the (cardinal) numbers 0 and 1.¹⁰ Finally,¹¹ he defines the ancestral of a relation, essentially as in *Begriffsschrift*; it will come as no surprise that he actually defines the operation which converts the extension of a relation into the extension of the ancestral of that relation. He then uses the ancestral, in application to the successor relation, to obtain the notion of a finite cardinal number.

The theorems whose proofs are given or sketched in *Grundlagen* are as follows. First, having defined the cardinality operator, Frege immediately proves that the statement 'There are just as many *F*s as *G*s' is equivalent to

⁵ Vol. I, § 37, p. 55.

⁶ § 38, p. 56.

⁷ § 39, p. 57.

⁸ § 40, p. 57.

⁹ § 43, p. 58.

¹⁰ §§ 41 and 42, p. 58.

¹¹ In §§ 45 and 46, pp. 59–60.

the statement 'The number of F s is the same as the number of G s' (§ 73).¹² It will be recalled that §§ 63–7 had been expended on an intricate discussion of whether it was possible to introduce the cardinality operator by stipulating that equivalence outright, terminating in a decision that it was not possible and in the consequent explicit definition of the operator. Now, having given that definition, Frege immediately exploits it to derive that very same equivalence. Furthermore, he never directly invokes the definition of the cardinality operator for any other purpose: all that follows depends only on the equivalence proved as the very first theorem. Crispin Wright devotes a whole section of his book, *Frege's Conception of Numbers as Objects*,¹³ to demonstrating that, if we were to take the equivalence in question as an implicit or contextual definition of the cardinality operator, we could still derive all the same theorems as Frege does. He could have achieved the same result with less trouble by observing that Frege himself gives just such a derivation of those theorems. He derives them all from that equivalence, with no further appeal to his explicit definition.

The proofs of the remaining theorems stated in *Grundlagen* present no difficulty, save for the last. The first (§ 75) states that if there are no F s and no G s, then the number of F s is the same as the number of G s, namely 0, and, conversely, that, if 0 is the number of F s, then there are no F s.¹⁴ There follow six theorems enumerated in § 78.¹⁵ Theorem (1) states that, if n is a successor of 0, then $n = 1$.¹⁶ Theorem (2) says that if 1 is the number of F s, then something is F , and theorem (3) that, on the same hypothesis, anything that is F is identical with anything that is F (in other words, that not more than one thing is F).¹⁷ Theorem (4) is a joint converse of these, saying that if at least one thing is F and at most one thing is F , then the number of F s is 1.¹⁸

We arrive at something more interesting with theorem (5), which states that the successor relation is one-one: this constitutes essentially the fourth Peano axiom.¹⁹ Theorem (6) says that any cardinal number other than 0 is a successor of some number.²⁰ The proposition that 0 is not a successor of anything, which is the third Peano axiom, is not expressly stated in *Grundlagen*, but is proved in *Grundgesetze*, Volume I.²¹ The last theorem stated in *Grundlagen* (§ 82), though not the last in the order of proof, is that no finite number follows itself in the series of natural numbers. That is to say that the sequence of natural

¹² The corresponding theorems of *Grundgesetze*, vol. I, are (32), § 65, p. 86, and (49), § 69, p. 93.

¹³ Aberdeen, 1983, section xix, pp. 154–69.

¹⁴ These appear in *Grundgesetze*, vol. I, as theorems 94, § 97, p. 128, and 97, § 98, p. 129.

¹⁵ These are listed together in symbolic form in *Grundgesetze*, vol. I, § 44, pp. 58–9.

¹⁶ This is theorem 110 of *Grundgesetze*, vol. I, § 103, p. 132.

¹⁷ The corresponding theorems of *Grundgesetze*, vol. I, are 113, § 103, p. 132, and 117, § 105, p. 133.

¹⁸ This is given in *Grundgesetze*, vol. I, as theorem 122, § 107, p. 136.

¹⁹ This forms theorem 90 of *Grundgesetze*, vol. I, § 95, p. 127.

²⁰ It figures in *Grundgesetze*, vol. I, as theorem 107, § 101, p. 131.

²¹ As theorem 108, § 103, p. 131.

numbers does not form a cycle, returning to itself.²² This theorem, as being the first listed here in which the generalisation is restricted to the natural numbers, is therefore also the first that requires an appeal to induction. It does not yet establish that the sequence of natural numbers is infinite, since it has to be shown that it does not terminate in a number that has no successor. To this purpose, Frege sketches in §§ 79 and 82–3 the proof of the crucial theorem that every natural (finite) number has a successor.²³ Every theorem so far is likely to be quite easily provable on the basis of anything worthy of the name of a construction of arithmetic; the infinity of the sequence of natural numbers, which, in Frege's construction, depends on the existence of a successor to every natural number, has a far more uncertain status. Since every number is, for Frege, the number of objects falling under some concept, he has to cite, for any given natural number n , a concept such that the number of objects falling under it is a successor of n . For this purpose, he takes the concept 'natural number $\leq n$ ' (where the relation $\leq$ is just the converse of the relation $\geq$ defined above). There is one number ≤ 0 , namely 0 itself, and the number 1 is a successor of 0. Likewise, there are two numbers, namely 0 and 1, each of which ≤ 1 ; and 2 is a successor of 1. In §§ 79 and 82–3, Frege shows how, by induction, to establish the general theorem that the number of natural numbers $\leq$ to any given natural number n is a successor of n ;²⁴ and from this the desired theorem that every natural number has a successor follows at once by existential quantification.

²² The theorem appears as 145 in *Grundgesetze*, vol. I, § 113, p. 144.

²³ *Grundgesetze*, vol. I, theorem 157, § 121, p. 150.

²⁴ *Grundgesetze*, vol. I, theorem 155, § 119, p. 149.

CHAPTER 11

Some Principles of Frege's Strategy

Contextual definition

The proposal discussed by Frege in *Grundlagen*, §§ 62–7, is to introduce the operator ‘the number of Φ s’, not by defining it explicitly, but by means of a contextual definition, namely by stipulating a sentence of the form ‘The number of F s is the same as the number of G s’ to be equivalent to ‘There are just as many F s as G s’, where the latter is in turn explained by an explicit definition in terms of one-one mapping; from § 64 onwards, the discussion is conducted in terms of the analogy with directions. In his middle period, Frege became hostile to contextual definitions, and to every form of definition other than straightforwardly explicit ones. It is quite evident, however, that, at the time of writing *Grundlagen*, he felt no such hostility, and, moreover, that he conceived of his context principle as licensing contextual definitions; his remark in § 60, that, in accordance with the context principle, it is sufficient that a sentence should have a sense as a whole, from which its parts derive their content, and that ‘this observation . . . throws light on many difficult concepts, such as that of the infinitesimal’, leaves no room for doubt on this score; this is reinforced by Frege’s reference to *Grundlagen* towards the end of his review of Hermann Cohen’s *Das Prinzip der Infinitesimal-Methode und seine Geschichte*. It is therefore unsurprising that Frege should treat the proposal for a contextual definition of the cardinality operator with complete seriousness.

His later objection to contextual definition was expressed in *Grundgesetze* by the use of a mathematical analogy.¹

It is evident that the reference of an expression and of one of its parts do not always determine the reference of the remaining part. One therefore ought not to define a symbol or word by defining an expression in which it occurs, the remaining parts of which are already known. For an enquiry would first be necessary whether any solution for the unknown – I here avail myself of a readily understandable algebraic metaphor – is possible, and whether the unknown is uniquely determined. As has already been said, however, it is impracticable to

¹ Vol. II, § 66.

make the justifiability of a definition depend upon the outcome of such an enquiry, which, moreover, may perhaps be quite unable to be carried out. The definition must, rather, have the character of an equation solved for the unknown, on the other side of which nothing unknown any longer occurs.

When he wrote *Grundlagen*, Frege plainly had not yet developed any objection to definitions whose justifiability depends on the proof of some proposition: he had spoken equably of such a possibility in § 3. Would he then have required, in order to justify a contextual definition, a proof that (to continue the metaphor) it had a solution, or, more exigently yet, that it had a unique solution? This, though very differently expressed, proved in the end to be his objection to the proposed contextual definition of the cardinality operator. Put more exactly, the contextual definition had a solution, but not a unique one; it had therefore to be replaced by an explicit definition, providing a determinate solution. There is no hint, in the text of *Grundlagen*, that from this any general objection to contextual definitions can be derived, and Frege's remarks in § 60 make it very doubtful that he thought so. It was just that this particular contextual definition, and others of analogous form, did not fulfil the requirements that we are entitled to demand of a definition.

The stipulation that the direction of a line a is to be the same as that of a line b just in case a is parallel to b does not determine whether the direction of a line is itself a line or something quite different: this contextual definition indeed has a solution, but it is far from unique. Even if the requirement were to be made that every direction should itself be a line, the stipulation would in no way determine which line any given direction was to be; it could, in fact, be any line whatever. A convenient choice would be to take some point as the origin O , and identify the direction of any line a with that line through O that was parallel to a ; even so, any point could serve for this purpose as the origin. The contextual definition might well be defended on the ground that we do not need to know anything about directions save what it tells us: as long as we know that the direction of a is the same as that of b just in case a is parallel to b , we are quite indifferent to what, specifically, the direction of a may be, or any other facts about it. Frege makes plain in § 66 that this defence would not satisfy him at all. It is an inexcusable defect in a proposed definition of the direction-operator that it fails to tell us what, specifically, the direction of a given line is to be: and hence it must be replaced by an explicit definition which does tell us that.

One reason, unacceptable to Frege, for giving a contextual definition, may thus be that it does not have a unique solution, and we do not wish to specify one. Another might be that, although the definition has a unique solution, we do not have the resources to specify it. There is, however, a quite different reason for framing a definition as a contextual one: namely, that the talk of a solution is quite misplaced. This will occur when the expression defined

contextually has a surface form belying that which the contextual definition assigns to it: the classic example is Russell's definition of the description operator. Russell permits this operator to retain the outward form of a term-forming operator of second level, that is, to be attached to a predicate. The whole point of the Theory of Descriptions, however, is to deny that definite descriptions, that is, the apparent terms formed by means of the description operator, are genuine terms at all. Given Russell's notation, it would therefore be impossible to give an explicit definition of the description operator, since any such definition would be forced to accept its apparent form as genuine. On Russell's theory, the description operator actually functions as a binary quantifier. Were Russell to have adopted a notation in which it explicitly figured as such, e.g. by writing

$$\text{Ix } [Fx, Gx]$$

for 'The *F* is *G*', there would be no obstacle to giving an explicit definition: the need for a contextual definition arises from the mismatch between the apparent and the real form of the expression defined. That is not at all the case with Frege's proposed contextual definition, however. The cardinality operator has the same overt form as Russell's description operator, namely that of a term-forming operator of second level: and Frege takes it without reservation to be of just that form. Numerical terms, including those formed by use of the cardinality operator, stand in all cases for objects.

Having framed the proposal for a contextual definition of the cardinality operator, Frege proceeds to consider three objections to it. The first two he rejects; the third, to the reader's surprise, he sustains, and so adopts instead his explicit definition. The first objection is stated in § 63 as follows:

The relationship of identity does not occur only among numbers. From this it seems to follow that we ought not to define it especially for this case. One might well think that the concept of identity would already have been fixed previously, and that from it and from the concept of cardinal number [*Anzahl*] it must follow when cardinal numbers are identical with one another, without there being any need for this purpose of a special definition besides.

This goes very much to the heart of the powerful, but obscure, principle of criteria of identity: does the principle require that identity be defined separately for each of a multitude of cases? Frege's answer to the objection contains a resounding 'No' to this question:

Against this it is to be remarked that for us the concept of cardinal number has not yet been fixed, but has first to be determined by means of our definition. Our intention is to construct the content of a judgement that can be regarded as an identity on either side of which a number stands. We are therefore not wishing

to define identity especially for this case, but, by means of the already known concept of identity, to attain that which is to be regarded as being identical. This admittedly appears to be a very unusual kind of definition, which has not yet been adequately noticed by logicians; but a few examples may show that it is not unheard of.

In the following section, § 64, Frege then gives various examples that he claims as analogous, including the concepts of direction and of shape; length and colour are mentioned in § 65. By presumption, what holds good for any one of these cases will hold good for all. Frege proceeds, in the same section, to state his grounds for holding that the concept of parallelism is prior to that of a direction, so that the latter must be defined in terms of the former, and not conversely; by analogy, the concept of a number must be defined in terms of cardinal equivalence, rather than defining 'There are just as many *F*s as *G*s' to hold when the number of *F*s and the number of *G*s coincide.

Frege's reply to the first objection can be seen only as an endorsement of the general principle of contextual definition, that procedure which, very similarly described, he repudiated in Volume II of *Grundgesetze*. The proposal is not to define the cardinality operator on its own, and then, by putting this together with the already known meaning of 'is the same as', to arrive at the sense of 'The number of *F*s is the same as the number of *G*s', by addition, as it were. Nor is it to *give* a sense to 'is the same as', but only for this special context. Rather, it is just *because* we already know what 'is the same as' means in all contexts that, by stipulating what 'The number of *F*s is the same as the number of *G*s' is to mean, we can arrive, by subtraction, at the meaning of 'the number of *F*s': the very procedure subsequently condemned by Frege in the passage cited above from *Grundgesetze*. In the earlier passage, there is no hint that he saw anything wrong with it.

But, if the sign of identity does possess a meaning which it retains in all contexts, and this meaning is already given, namely by Leibniz's law, as Frege explains in § 65, a second objection arises: how can we be sure that our definition does not conflict with the general laws of identity? The objection cannot receive a general answer: we need to examine the particular proposed contextual definition; in § 65, the matter is being discussed apropos of the contextual definition of the direction-operator in terms of the relation of being parallel. For this case, we need specifically to show, for any lines *a* and *b*, that, if *a* is parallel to *b*, the term 'the direction of *a*' can be replaced, in all contexts, by 'the direction of *b*' without change of truth-value. Frege here remarks that we have not as yet provided for the occurrence of such terms in any other context than an identity-statement connecting two of them. For such contexts, the question reduces to one of showing parallelism to be an equivalence relation, that is, reflexive, transitive and symmetric. This, of course, is readily done; but it is necessary if the contextual definition of the direction-operator

is to be justified: the demonstration is one of those securing the legitimacy of a definition of which Frege had spoken in § 3. By analogy, the proposed contextual definition of the cardinality operator would need to be justified by showing cardinal equivalence – the relation expressed by ‘just as many . . . as’ – to be an equivalence relation of second level (an equivalence relation between concepts).

Frege here commits a blunder, easily overlooked. Having observed that ‘we initially know of nothing else that can be predicated of the direction of a line than that it coincides with the direction of another line’, Frege adds:

All other statements about directions would first have to be defined, and for these definitions we can impose the rule that the intersubstitutability of the direction of a line by that of another one parallel to it must be ensured.

It is natural to regard this remark as obvious. If it is worth introducing terms for directions at all, we shall surely want to say things about directions other than that they are or are not identical. To do this, we must introduce suitable predicates. This we shall surely do by means of further contextual definitions, equating statements assigning properties to, or relations between, directions with ones assigning corresponding properties to, or relations between, lines. For such a definition to be legitimate, the relation of parallelism must be a congruence relation with respect to the property of, or relation between, lines that it invokes: and therefore we shall be involved in giving a chain of contextual definitions, one for each context in which we want terms for directions to occur, and, with it, a chain of proofs that parallelism is a congruence relation with respect to various properties and relations.

For example, we might propose to define the direction of a to be *orthogonal* to the direction of b just in case the line a is perpendicular to the line b . This definition would be in order if we were concerned only with plane geometry, so that all the lines considered were on a single plane: but, to show it to be in order, we should have to show that being parallel was a congruence relation with respect to perpendicularity. That is, we must show that, if a is parallel to a' , and b to b' , and if, further, a is perpendicular to b , then also a' is perpendicular to b' . If, however, we were concerned with three-dimensional space, we should not be able to prove that, since two lines can be perpendicular only if they intersect; and so the definition would be inadmissible.

In the same way, we shall wish to be able to say, not merely that cardinal numbers n and m are equal to one another (that is, are identical), but also that one is less than or equal to another in magnitude; as is well known, we shall then need to define ‘less than’ to mean ‘less than or equal but not equal’. We shall also need to define the operations of cardinal arithmetic – addition, multiplication and exponentiation. To take the first step, we may well define the number of F s to be less than or equal to the number of G s just in case

there is a one-one map of the *F*s into the *G*s, that is, on to some (possibly all) of the *G*s. To justify this definition, we must show that cardinal equivalence is a congruence relation with respect to mapping into: if there are just as many *F*s as *H*s and just as many *G*s as *K*s, and the *F*s can be mapped one-one into the *G*s, then the *H*s can be mapped one-one into the *K*s.

It is, however, a mistake to suppose that, once the original contextual definition has been given, any further such definitions are needed. We need only define a direction *p* to be orthogonal to a direction *q* if there exist lines *a* and *b* such that *a* is perpendicular to *b*, and *p* is the direction of *a* and *q* the direction of *b*, and then our definition is unquestionably legitimate, without the need for any proof. This will not, of course, save us any real work: we shall still need to prove, on the plane, that, if the direction of *a* is orthogonal to the direction of *b*, then *a* is perpendicular to *b*, something that will not be so, on this definition, in 3-space. But it serves to bring out the force of the principle which Frege's insight lighted on, that, in determining concepts like number and direction, the criterion of identity is the first feature to be fixed. We might well question this for cardinality. To lay down when two sets are to be said to have the same number of members is well known not to determine unambiguously when one should be said to have fewer members than, or at most as many members as, another. If we defined in the usual way when there were at most as many *F*s as *G*s, on the other hand, we could stipulate that there were just as many *F*s as *G*s if there were at most as many, and also at most as many *G*s as *F*s; the Schröder-Bernstein theorem would guarantee that the notion so defined would coincide with the usual one, as defined in terms of mappings on to. This might well lead us to think that cardinal equivalence is not after all the fundamental notion.

Such a thought would, at least in one clear sense, be mistaken. It is true that we need an independent definition of the binary quantifier 'There are at most as many *F*s as *G*s'. It is also true that we could, if we liked, define 'There are just as many *F*s as *G*s' from it. But the latter notion – that of cardinal equivalence – is still what we need in order to arrive at cardinal numbers, that is, to introduce the cardinality operator, whether by means of a contextual definition or otherwise: and, when we have it, we need no contextual definition or alternative device for defining ordering relations by magnitude between cardinal numbers. Just as in the case of directions, we can define a number *n* to be less than or equal to a number *m* just in case there exist concepts *F* and *G* such that there are at most as many *F*s as *G*s, and *n* is the number of *F*s and *m* the number of *G*s. By contrast, if we had tried to introduce the cardinality operator by the contextual stipulation that 'The number of *F*s is less than or equal to the number of *G*s' was to be equivalent to 'There are at most as many *F*s as *G*s', we should have been unable to advance: whether 'There are just as many *F*s as *G*s' had been defined in terms of 'There are at most as many *F*s as *G*s' or in the usual way, directly, we should be quite

unable to prove the number of *F*s to be the same as the number of *G*s when there were just as many of the one as of the other: in this case, we should have no choice but to make a second contextual stipulation. It is true that we cannot avail ourselves of the same device of existential quantification when we come to define the operations of cardinal arithmetic. In that case, we must justify our definitions by proving cardinal equivalence to be a congruence relation with respect to analogous operations on sets (in Frege's terms, on concepts). But it is still cardinal *equivalence* that is the pivot on which the definitions turn.

Why numbers had to be objects

It is only when we have surveyed the chain of definitions Frege actually gives, and of theorems he actually proves, that we can see why he had, in *Grundlagen*, § 56, to reject a radical adjectival strategy; why, that is, he had to take sentences containing apparent singular terms for numbers at face value. Contrary to the impression he contrives to convey in §§ 56–7, the radical strategy can be pursued for a considerable distance: the definitions and proofs Frege actually gives in §§ 71–83 can readily be mimicked in the adjectival mode. To illustrate this, some laxity of notation is desirable: if we were to retain the bound variables needed to satisfy Frege's principle that no concept-expression ought ever to appear deprived of its argument-place, the formulas would become too cluttered with bound variables to be readable. For purposes of exposition, therefore, we may, when convenient, omit the argument-places, writing ' $\exists_1 (F)$ ' for 'There is just one *F*', ' $F \approx G$ ' for 'There are just as many *F*s as *G*s', and so on.

As already indicated, we may start with the analogue of the predicate 'is a cardinal number'. Where '*M*' ranges over second-level concepts, we may use ' $\text{Card}(M)$ ' to mean that ' $M(F)$ ' holds good just in case *F* is of some one particular cardinality. ' $\text{Card}(M)$ ' may thus be defined to mean:

$$\forall F [M(F) \rightarrow \forall G (M(G) \longleftrightarrow F \approx G)]$$

The numerically definite quantifiers ' $\exists_0$ ' and ' $\exists_1$ ' may now be defined just as in § 55. We may use ' $\text{Succ}(N, M)$ ' as the analogue of '*n* is a successor of *m*', and define it to mean:

$$\forall F [N(F) \rightarrow \exists x (Fx \ \& \ M_y (Fy \ \& \ y \neq x))]$$

Now, where '*K*' ranges over *third*-level concepts, and '*P*', '*Q*' over second-level ones, we may define ' $N > M$ ' to mean:

$$\begin{aligned} &\forall K [(\forall P (\text{Succ}(P, M) \rightarrow K(P)) \ \& \\ &\forall P \forall Q (K(Q) \ \& \ \text{Succ}(P, Q) \rightarrow K(P)) \rightarrow K(N)] \end{aligned}$$

' $N \geq M$ ' may then be set as:

$$N > M \vee \forall F [N(F) \longleftrightarrow M(F)]$$

Finally, the analogue of ' n is a finite number' may be written as ' $\text{Nat}(N)$ ', and defined to mean:

$$N \geq \exists_0.$$

With these definitions, we may readily prove the analogues of all the theorems proved by Frege up to *Grundlagen*, § 78, together with that of the theorem stated in § 83, that, for every natural number n , $\neg n > n$. The analogues of the remaining two theorems cannot be proved, however. These are the theorem establishing the infinity of the sequence of natural numbers, to the effect that every natural number has a successor, and its lemma, saying that, if n is a natural number, the number of numbers m such that $n \geq m$ – that is, of numbers from 0 to n inclusive – is a successor of n . Since, according to the radical adjectival strategy, we are not taking numbers to be objects, we cannot so much as frame the lemma. In place of numbers, we have (numerical) second-level concepts. To say how many second-level concepts there are satisfying a given condition – to say, for example, for given N , how many second-level concepts M there are such that $N \geq M$ – we should need an expression for a fourth-level concept, T , say. But this T could not be a successor of N , for they are concepts of different level, and it is only of a concept of second level that we can meaningfully say that it is a successor of some other second-level concept. Now, since numbers are not being taken to be objects, our theory will not contain any presumption that there are infinitely many objects: it will be perfectly consistent with the hypothesis that there are, say, only 100. If there were only 100 objects, the second-level concept $\exists_{100}$ would have no successor; for, if it had a successor, the condition

$$\exists x \exists_{100} y (Fy \ \& \ y \neq x)$$

would have to hold for some suitable F , and there would then after all be at least 101 objects. It was for precisely the same reason that Russell, whose theory of types required that numbers, as classes of classes, be segregated from individuals, was compelled to adopt an Axiom of Infinity, stating that there were infinitely many individuals, in order to guarantee that there were infinitely many cardinal numbers; in fact, if value-ranges are excised from the logical system of *Grundgesetze*, the result is a form of the simple theory of types. It is only because Frege reckoned numbers among objects, that is, as

belonging to the domain of the individual variables, that he was enabled to spin the infinite sequence of natural numbers out of nothing, as it were. There must be at least 0 objects, and hence the number 0 exists. Since the number 0 exists, there is at least one object, and so the number 1 exists: and so on indefinitely. It is in order to prove the infinity of the natural-number sequence that Frege is compelled to construe numbers as objects, and not for any of the spurious reasons he cites in §§ 56–7.

It may naturally be said that there can be no such thing as a purely logical proof that there are denumerably many objects – logical objects – unless a strong existential assumption was built into the theory at the outset. Existential assumptions, when not explicitly stated by means of the existential quantifier, are incorporated into a theory by the use of term-forming operators and the principles assumed to govern them. In the theory of *Grundlagen*, this is done by use of the cardinality operator, or, when this is defined in terms of classes, by the abstraction operator by means of which class-terms are formed. If there are n objects altogether, there will be $\theta(n)$ cardinal numbers, for a suitable function θ : when $n \leq \text{Aleph-0}$, we may put $\theta(n) = n + 1$. Since the cardinal numbers are themselves objects, we must have $\theta(n) \leq n$; and this can be so only when $n \geq \text{Aleph-0}$. In a similar way, Frege's use in *Grundgesetze* of the abstraction operator for forming value-range terms imposed a restriction on the cardinality of the domain. If there are n objects in the domain, there will be n^n value-ranges; since value-ranges are objects, we must have $n^n \leq n$. There is only one value of n for which this inequality holds, namely $n = 1$; but since Frege had assumed the existence of at least two objects, the two truth-values, a contradiction resulted, and the system was inconsistent.

From this we see more clearly the content of Frege's thesis that numbers are objects. The argument used in § 57, from the surface appearance of numerical terms in arithmetical statements, is in any case weak, since it is not mandatory to respect surface form. Even if it were allowed full weight, however, it would prove too little. We could respect the surface forms of arithmetical statements in a two-sorted theory, distinguishing, say, a domain of actual (*wirkliche*) objects from a domain of non-actual ones, or of non-logical objects from a domain of logical ones. If the cardinality operator were allowed to be attached only to predicates applying to objects of the first domain, we could not form the term 'the number of numbers m such that $n \geq m$ ', and so could not prove that there were infinitely many natural numbers. If we were permitted to form terms by attaching the cardinality operator to predicates applying to objects of the second domain, in which the numbers were located, then of course we could prove the infinity of the natural number-sequence just as Frege does; but there would then be little point in distinguishing the two domains. Frege's thesis that numbers are objects does not imply merely that expressions for numbers have the logical status of singular terms: it implies, further, that such terms stand for objects belonging to the sole domain over

which the individual variables range. Such objects therefore fall within the scope of the first-level quantifiers. Frege's explanation of the universal quantifier in *Grundgesetze* is of a resolutely 'objectual' character, in Quine's terminology:²

$$\forall a \Phi(a)$$

is to denote the value *true* if the value of the function $\Phi(\xi)$ is the value *true* for every argument, and otherwise the value *false*.

In order that quantified statements should have a determinate truth-value, all the objects in the domain must be, as it were, already in place, independently of which of them is denoted by some term that can be framed in the formal language.

This bears on the question whether the modes of introducing the direction-operator and the cardinality operator reviewed, and ultimately rejected, in §§ 63–7 of *Grundlagen* are genuinely contextual *definitions*, in the standard sense of permitting the elimination of the defined expression, by transforming any sentence containing it into an equivalent one not containing it. If the proposed contextual definition of the direction-operator is construed as introducing a two-sorted language, with one sort of individual variable ($a, b, \dots$) ranging over a domain of lines and a new sort ($p, q, \dots$) ranging over a domain of directions, in which the direction-operator can be attached only to a term or variable for a line, the elimination is easy. If we write ' $\text{dir}(\xi)$ ' for 'the direction of ξ ', we have first to transform any quantification over directions:

$$\forall p (\dots p \dots)$$

into a quantification over lines:

$$\forall a (\dots \text{dir}(a) \dots).$$

Then any subformula of the form

$$\text{dir}(a) = \text{dir}(b)$$

can be replaced by

$$a // b.$$

Suppose that we have defined ' p is orthogonal to q ', written as ' $p \perp q$ ', to mean

² Vol. I, § 8. In this quotation I have used the standard modern symbol for the universal quantifier, in place of Frege's concavity.

$\exists a \exists b (p = \text{dir}(a) \ \& \ q = \text{dir}(b) \ \& \ a \text{ is perpendicular to } b).$

We may then convert

$$\text{dir}(c) \perp \text{dir}(d)$$

into

$\exists a \exists b (a // c \ \& \ b // d \ \& \ a \text{ is perpendicular to } b).$

In this manner, the expanded language, involving reference to and quantification over directions, can be translated into the original language, involving only reference to and quantification over lines.

Such a two-sorted theory provides the only context in which the stipulation of the criterion of identity for directions genuinely constitutes a contextual *definition*, in the sense of one supplying a means of eliminating any occurrence of the expression defined. It certainly does not represent Frege's intention, however: he surely wished to add the direction-operator to a one-sorted language whose individual variables ranged over lines, and many other objects besides, and whose single domain would then be taken also to include directions, and tacitly to have included them all along. The vagueness of the background against which the proposed contextual stipulation is supposed to be given makes it difficult to discuss; but we may simplify our discussion by supposing that, in the original language, the variables were capable of being interpreted as ranging only over lines. Now, in the language expanded by the addition of the direction-operator, we have two choices, if we are not to abandon at the outset all chance of eliminating that operator: we may either identify directions with lines, or differentiate each from the other. We may begin by considering the first of the two options. It would be entirely contrary to Frege's principles to place any restrictions on the occurrence of terms for directions in the argument-places of the already existing predicates. Hence, if we are hoping to eliminate such terms from all contexts, we must add assumptions strong enough to identify specifically the line with which any given direction is to be equated, say by the device, already mentioned, of taking the direction of a to be the line through some particular point O parallel to a . We shall, in other words, have to meet Frege's third objection, and lay down enough to determine, not indeed whether England is the direction of the Earth's axis, since, in our artificially restricted example, we have the means of referring only to lines and their directions, but whether the Earth's axis is or is not the same as its direction. If we do this, however, the 'contextual' stipulation

$$\text{dir}(a) = \text{dir}(b) \longleftrightarrow a // b$$

will become otiose, since we shall be able to derive it as a theorem; moreover, we shall be in a position to define the direction-operator explicitly.

The alternative is the stipulation that, for every x , the direction of x is not a line. To formulate this within the theory, we should have to introduce a predicate, 'Line(ξ)', meaning ' ξ is a line', and lay down a number of axioms serving in effect to give the one-sorted theory the force of a two-sorted one. One such axiom would be:

$$\forall x \neg \text{Line}(\text{dir}(x)).$$

Another, to sterilise reiterations of the direction-operator, might be:

$$\forall x (\neg \text{Line}(x) \rightarrow \text{dir}(x) = x).$$

Furthermore, for each predicate of lines, we should need to adopt an axiom of the form:

$$\forall x \forall y (x // y \rightarrow \text{Line}(x) \ \& \ \text{Line}(y)).$$

By these means, we should obtain a theory whose theorems might be translated into theorems of the original theory which contained no direction-operator. We should nevertheless be unable to eliminate the direction-operator by proving, within the new theory, the equivalence of statements containing it with statements not containing it. The reason is that, in the process of mapping theorems of the new theory into theorems of the old, we should not be able to leave the quantifiers intact, but should have to translate them. Thus a statement of the form ' $\forall x A(x)$ ' would first have to be transformed into:

$$\forall x [\text{Line}(x) \rightarrow (A(x) \ \& \ A(\text{dir}(x)))].$$

When ' $A(x)$ ' had been transformed into a formula ' $B(x)$ ' not containing the direction-operator, and ' $A(\text{dir}(x))$ ' into another such formula ' $C(x)$ ', we could finally render the statement ' $\forall x A(x)$ ' of the new theory as the statement

$$\forall x (x // x \rightarrow B(x) \ \& \ C(x))$$

of the old one. Similarly for the existential quantifier. For instance, in the new theory we could trivially prove:

$$\exists x \neg \text{Line}(x).$$

This we should first have to transform into:

$$\exists x [\text{Line}(x) \ \& \ (\neg \text{Line}(x) \vee \neg \text{Line}(\text{dir}(x)))].$$

Since ' $\text{Line}(x) \ \& \ \neg \text{Line}(x)$ ' is a contradiction, and ' $\neg \text{Line}(\text{dir}(x))$ ' is a consequence of the axioms that had been added, this would reduce to:

$$\exists x \text{Line}(x),$$

which could be rendered in the language of the original theory as

$$\exists x \ x // x.$$

Plainly, such a transformation would not count as a mere elimination of the direction-operator in the usual sense, since it would involve tampering with the (one-sorted) variables of quantification in the passage from the new theory to the old one.

Thus even the stipulation of the criterion of identity for directions cannot count as a contextual definition proper save against the background which Frege undoubtedly did not intend. This, it may be said, explains why Frege rejected it, his programme being to demonstrate arithmetical statements to be analytic in the sense of being reducible to logical truths with the aid of *definitions*. In any case, it does not differentiate the supposed contextual definition of the direction-operator from that of the cardinality operator; for that, too, would become a genuine contextual definition only if it were part of a two-sorted theory. Suppose we have a second-order, one-sorted language, with individual variables $x, y, \dots$, in which is expressed a theory of any kind you please. To this we add individual variables of a second sort, namely number-variables $m, n, \dots$, and the cardinality operator, construed as forming terms denoting numbers (elements of the second domain) when attached to predicates applying to elements of the original domain. The cardinality operator ' $\text{card}_x [\Phi x]$ ' will be governed by:

$$\approx_x [Fx, Gx] \longleftrightarrow \text{card}_x [Fx] = \text{card}_x [Gx].$$

We can now translate every statement of this expanded theory into a statement of the original theory, first rendering a quantification of the new sort:

$$\forall n \ A(n)$$

as

$$\forall F \ A(\text{card}_x [Fx]).$$

In such a theory, we could not prove that there are infinitely many natural

numbers, since we could not even frame a term for the number of numbers less than or equal to a given number n : the cardinality operator can be attached only to predicates applying to the original objects, not to those applying to numbers. But that only shows what we already knew, that such a two-sorted theory was not what Frege had in mind.

Eliminability of the newly introduced operator is not the principal point, however. Even though, in a one-sorted theory, the criterion of identity for directions is not a contextual definition, properly so called, we have a ready means of constructing a model of the new theory, given a model of the original one; the easiest way to do so is that already canvassed, by identifying directions with lines. That is made possible by the ontological parsimony of the theory of directions: it does not demand the existence of any more of the new objects – directions – than there were of the old ones – lines. The theory of cardinal numbers is very far from being ontologically parsimonious, on the other hand: it requires the existence of $n + 1$ new objects – numbers – given n objects of the original kind, and hence, in a one-sorted theory such as Frege intended, of the original kind and the new kind taken together. If the model for the original theory was finite, a denumerable model would indeed suffice for the expanded theory to which numerical terms had been added; but that would obviously not be a model for whose construction we had employed only the resources required for the construction of the original one. Even if we could not in the usual sense eliminate the direction-operator, we could, by reinterpreting the quantifiers, translate statements involving directions into statements not involving them. We cannot do this for statements involving numbers. Since the cardinality operator is of second level, occurrences of it can be embedded within the scope of other occurrences in a much more complicated way than could happen with the first-level direction-operator. Consider the crucial term ‘ $\text{card}_m [n \geq m]$ ’, essential for the proof of the infinity of the sequence of natural numbers. The relation $\geq$ between n and m is the ancestral of the successor relation, whose definition involves two occurrences of the cardinality operator. No programme of eliminating the cardinality operator by appeal to the supposed ‘contextual definition’ – the criterion of identity for numbers – could possibly succeed in eliminating these inner occurrences of the operator. The reason is that they will be found to stand in contexts of the form

$$\text{card}_x [Fx] = k,$$

where not only ‘ F ’ but ‘ k ’ is a bound variable; and the ‘contextual definition’ provides no means of reducing an equation in which a numerical term stands on one side and a variable on the other. No alternative strategy of definition could have avoided this, if Frege’s proof of the infinity of the sequence of natural numbers was to go through. This may be seen as a special case – indeed, the crucial case – of Frege’s third objection.

The fact that the cardinality operator is of second order, while the direction-operator is of first order, thus proves to be no irrelevancy, as Frege would have us think, but of critical importance: it is just for that reason that the introduction of the cardinality operator embodies a far stronger ontological assumption, namely that the domain of objects over which our individual variables range is infinite. That is what is involved in regarding numbers as objects: to treat it as embodying that assumption was a heavy burden for Frege to have allotted to logic. Scattered amongst his writings are muted invocations of an argument for taking numbers to be objects, different from those he states in *Grundlagen*. This is that numbers can themselves be counted, and that in number theory we often need to speak of the number of natural numbers satisfying a given condition. Even on Frege's principles, it does not directly follow from the fact that numbers can be counted that they must be objects, on the ground that a cardinal number is the number of objects falling under some first-level concept; for Frege observes more than once that concepts, too, can be counted. There is more force in the observation that we frequently wish to relate a natural number to the number of numbers standing to it in a certain relation, as when we define Euler's number-theoretic function $\phi(n)$ to be the number of numbers $\leq n$ and prime to it. But the fact is that even this argument is not compelling. In *Grundgesetze*, Frege admits binary functions, and therefore relations, whose arguments are of different levels;³ there is therefore no reason why we should not consider a one-one mapping of things of one level on to those of another. Functions such as $\phi(n)$ may be dealt with even more conveniently. We need only define the characteristic function $\chi(m, n)$ for the relation 'are co-prime to one another', putting

$$\chi(m, n) = \begin{cases} 1 & \text{if } (m, n) = 1 \\ 0 & \text{otherwise} \end{cases}$$

and then defining $\phi(n)$ by:

$$\phi(n) = \sum_{m=1}^n \chi(m, n).$$

This could also be written, without appeal to $\chi(m, n)$, as:

$$\phi(n) = \sum_{\substack{0 < n < m \\ (m, n) = 1}} 1.$$

³ Vol. I, § 22, p. 38.

This leaves Frege without a proof that numbers are objects: only a strong motive for taking them as such.

CHAPTER 12

Frege and Husserl

How sound was Frege's strategy of definition? We have seen him left with a motive, but no justification, for taking numbers to be objects; but perhaps the motive was justification enough in itself. If number theory requires the existence of infinitely many objects, it is more appropriate to take those objects to be numbers, as Frege did, than to presuppose, with Russell, the existence of infinitely many non-logical objects. This apart, of all possible strategies, the decision to treat numbers as objects rules out only one, the radical adjectival strategy according to which the surface appearance of arithmetical statements, with the apparent numerical terms they contain, is illusory; every other means of connecting the adjectival and the substantival uses of number-words, every other choice of which notions are prior and which to be defined in terms of those, is left a possibility by that decision. Having read *Grundlagen*, one may well believe that Frege has shown his definitional strategy to be mandatory; but that only reflects his skill in presenting his task, at each stage, so that the step he actually takes appears inevitable; in fact, he argues far less in favour of the route he adopts than the reader has the impression that he does. Even for the conceptual priority of cardinal equivalence over the notion of a cardinal number, he argues, as we have seen, only in terms of a faulty analogy. The justification of the strategy he follows is therefore best studied by considering the objections of a critic. Seven years after *Grundlagen* appeared, Edmund Husserl published his *Philosophie der Arithmetik*, in which Frege's book is subjected to strong criticism, not on the ground that the definitional equivalences were false, but on the ground that they flouted the true relations of priority between the concepts involved. Three years later yet, Frege reviewed Husserl's book, vigorously retorting to his criticisms. The exchange provides a perfect basis for our enquiry.

Husserl's objections

In Chapter VI of his book, Husserl first objects that, considered as a *definition* of identity, which is how Frege presents it in *Grundlagen*, § 65, Leibniz's law

'stands the true state of affairs on its head', as he expresses it, borrowing Frege's own phrase.¹ The question is not one of the extensional correctness of the definition, but of conceptual priority: the only ground for assuming the replaceability of one content by another in all true judgements, Husserl argues, is their identity; we are therefore not entitled to *define* their identity as consisting in that replaceability. Husserl is in fact mistaken in contending that Frege made the Leibnizian definition of identity basic to his construction of the concept of number; although Frege allowed it as a genuine definition, all that concerned him was that the *laws* of identity consequent upon it should hold, and, further, that the equals sign in mathematics be construed as denoting strict identity. That is why, in his review,² he was able to concede that Leibniz's law does not constitute a definition.

Husserl's remarks about Leibniz's law serve to illustrate his concern, in a great part of his discussion, precisely with the question of conceptual priority, that is, with the question which of some pair of notions should be taken as serving to explain the other. As we have seen, Frege, in his review of Husserl's book, ignores such questions altogether, blandly maintaining that the only criterion for a correct definition in mathematics is that it maintain the reference of the defined expression. In *Grundlagen*, however, he had given it a central place in his argument, contending, in § 64, that we must on these grounds explain 'The direction of *a* is the same as the direction of *b*' as meaning the same as '*a* is parallel to *b*', and that the converse order of explanation, defining parallelism as identity of direction, would 'stand the true state of affairs on its head'. Plainly, extensional correctness is not here the consideration, either: the correctness of the equivalence is taken for granted, the problem being the proper direction of explanation. 'What is essential in this discussion can easily be transferred to the case of numerical identity', he tells us in the first footnote to § 65; we must therefore assume that Frege was tacitly also contending that conceptual priority required that we explain 'The number of *F*s is the same as the number of *G*s' as meaning the same as 'There are just as many *F*s as *G*s', and not conversely. His answer to Husserl in the review was disingenuous, at least as regards his intentions when he wrote *Grundlagen*.

Husserl goes on in his Chapter VI to discuss the definition of equinumerosity in terms of one-one mapping. This definition, given by Frege in full in §§ 70–2 of *Grundlagen*, was not original with him; in § 63 he attributes it to Hume, as well as citing uses of it by Kossak in 1872, Schröder in 1873 and Cantor in 1883. In fact, Cantor had used it as early as 1874; Husserl cites Schröder and Stolz (1885) as well as Frege. By the time that Frege wrote *Grundlagen*, the definition had already become a piece of mathematical orthodoxy, though Frege undoubtedly gave it its most exact formulation and its most acute

¹ Review of Husserl, p. 320.

² E. Husserl, *Philosophie der Arithmetik*, Halle, 1891, p. 104.

philosophical defence. Nevertheless, Husserl, in 1891, thought proper to attack it.

The paradox of analysis

As before, Husserl does not contest the extensional correctness of the definition, or even deny that this extensional equivalence is a truth of logic: he admits³ that 'it lays down a *necessary and sufficient condition in the logical sense*, valid in all cases, for the obtaining of equality'. But this is not enough, in his view, for the definition to be admissible. The possibility of a one-one mapping of one plurality on to another does not, he says, '*constitute* their equinumerosity, but only *guarantees* it': 'to know that their numbers are equal does not in the least require knowing that it is possible' to map one on to the other, and so 'the one piece of knowledge is in no way identical with the other'. Here Husserl is at the threshold of the paradox of analysis that so exercised G.E. Moore: it is precisely the problem, which we left in abeyance, of the status of those analytic definitions with which *Grundlagen* abounds, but of which, as we saw earlier, Frege failed ever to give a satisfactory account. The notion expressed by sentences of the form 'There are just as many *F*s as *G*s' is a commonplace one: in what sense is it to be analysed in terms of that of a one-one mapping, as explained in detail by Frege in §§ 71–2? Surely someone can understand the phrase 'just as many' without having the remotest idea of what a one-one map may be; how, then, can an explanation of his understanding by appeal to one-one mappings possibly be correct? Should we say that the explanation brings to light what he tacitly, but not overtly, knows? Or should we, rather, say with Husserl that it merely sets out a logically necessary and sufficient condition, without penetrating to that in which his understanding actually consists?

Counting

While Frege blurred the distinction between defining the cardinality operator in terms of equinumerosity, and defining equinumerosity in terms of one-one mapping, Husserl ignored it altogether; and this makes it difficult to discern the precise intention of his criticisms. One natural interpretation is that he objected to the adjectival-to-substantival direction adopted by Frege for introducing the cardinality operator, believing that 'There are just as many *F*s as *G*s' should be explained as meaning that the number of *F*s and the number of *G*s are the same rather than conversely. Husserl lists three means by which equinumerosity may be established. The first of these can be understood only in the light of Husserl's espousal of the abstractionist theory of numbers as

³ Op. cit., p. 114.

sets of units demolished by Frege in §§ 33–44 of *Grundlagen*; in the fifth section of Chapter VIII of his book, Husserl discussed Frege's criticisms of the theory, but it is hard to see how he can have supposed that he had met them. The first of Husserl's three methods of establishing two pluralities to be equinumerous begins with the psychological act of abstraction; having reduced each plurality to a set of units, we may map the units in the first set one-to-one upon those of the other. The second method is much simpler: we apply the operation of one-one mapping directly to the original concrete sets, without any prior act of abstraction. This second method is, therefore, precisely that which Husserl is denying to contain the very meaning of 'equinumerous'. It is, however, the third method which is preferable to either of the other two, both as yielding more information and as corresponding to what 'just as many' means: we count the members of each plurality, thereby determining not only whether there are just as many elements of the one as of the other, but, specifically, the number of elements in each; 'the simplest criterion for equality of number is just obtaining *the same number* when one counts the sets to be compared'. Husserl's thesis closely resembles the answer that a child would give when first asked the question, 'What does it mean to say that there are just as many nuts as apples in the bowl?'; almost any child will reply, 'It means that, when you count each of them, you will get the same number'.

Frege has no difficulty, in his review, in disposing of these objections. The second method just is that of establishing a one-one mapping, while the first uses such a mapping indirectly. So, however, does the third: 'the author forgets that counting itself rests on a one-one correlation, namely between the number-words from 1 to n and the objects of the set' (p. 319). This retort is evidently wholly justified. Our sequence of number-words, as we employ them in counting, forms a kind of universal tally with respect to which we can compare the cardinalities of different concepts, and thus provides a means of giving, in the finite case, a specific answer to the question 'How many are there?'. Specific answers to questions of forms such as 'How long?', 'How far?', 'How heavy?', etc., always demand the adoption of a conventional standard of comparison: the question 'How many?' appears to be no exception. Husserl obviously has in mind only finite sets. To accommodate the case of infinite sets, therefore, his thesis might be generalised to the following: to say that there are just as many F s as G s is to say that a definite answer to the question, 'How many F s are there?', will be the same as a definite answer to the question, 'How many G s are there?'. Unfortunately for defenders of Husserl, this does not resolve the difficulty: for it is only by appeal to the notion of equinumerosity that we can explain what constitutes a definite answer to a question 'How many?'.

Abstraction

If we construe Husserl's argument as previously suggested, namely as favouring a definition of 'There are just as many *F*s as *G*s' in terms of the cardinality operator, viz. to mean 'The number of *F*s is the same as the number of *G*s', his tactics were at fault. The whole point of the abstractionist theory was to explain how one arrives at the same number if one starts from any set of a given cardinality: the number being a set of featureless units, attained by abstracting from the characteristics of the elements of the original set, no difference can appear between any one such abstract set and another save how many such units it contains. Husserl indeed concedes that the act of abstraction, as performed in his first method of establishing numerical equivalence, does not yield the actual number of objects in each plurality. Presumably this must be interpreted as meaning that, although we have attained the number, we do not yet know which number it is: we are in the position of Alice when the White Queen tested her knowledge of addition by asking, 'What is one and one and one and one and one and one and one and one and one and one and one?'. In this respect, however, Husserl's theory is at no disadvantage as against Frege's, according to whose definitions 'There are four horses in the field' is tantamount to 'There is one more than one more than one more than one horse in the field'. The act of abstraction by which we pass from a set to the number of its members involves no reference to any one-one mapping, or indeed to a comparison of any kind of the original set with any other; and so it explains in what the number of members of the set consists without appeal to such a comparison. Having arrived in this manner at the number of *F*s and at the number of *G*s, there will be no need to *correlate* the units belonging to one with those belonging to the other, as in Husserl's first method of establishing numerical equality: for the two sets of units will be strictly identical.

If the procedure of abstraction had really worked as it was supposed to do on this theory, Husserl would have had a perfect rebuttal of the definition in terms of one-one mapping. One powerful argument for a thesis that one notion is conceptually prior to another is the possibility of defining the first without reference to the second. Frege has to hand a definition of equinumerosity independent of the cardinality operator, and he tacitly denies the possibility of defining the cardinality operator independently of equinumerosity; if the abstractionist theory had been sound, 'the number of *F*s' could have been explained without reference to equinumerosity, and the scores would have been equal on this count. A second argument for conceptual priority is greater simplicity: since it is obviously simpler to explain 'the number of *F*s' without considering any comparison of the *F*s with the *G*s, for some other concept *G*, Husserl's team would thereby have defeated Frege's. Had it been possible to sustain the abstractionist theory, such a victory could have been secured.

Why, then, did Husserl not adopt these tactics? He did not see the full

force of Frege's refutation of abstractionism; but he saw enough to grasp that the units, although featureless, must in some way retain their distinctness, in the form of some shadowy remnant of the particular objects from which they had been engendered; and so he spoke of correlating like-numbered sets of units instead of merely registering that they are identical, without observing that, with this concession, the entire abstractionist theory falls to the ground.

Another interpretation of Husserl

There is, however, an alternative way of interpreting Husserl's discussion, as arguing, not for the definition of an adjectival notion, that of equinumerosity, in terms of a substantival one, that of cardinal numbers, but for a reversal of the downward direction of Frege's sequence of definitions, as set out in the table in Chapter 10. In particular, Frege's official definitions explain each numerically definite quantifier in terms of the cardinality operator and the corresponding cardinal number: 'There are two apples on the table' is explained as meaning 'The number of apples on the table is 2'. Since the cardinality operator and the individual numbers are defined in terms of equinumerosity, this means that 'there are just two' and the rest are defined, ultimately, in terms of 'just as many'; and Husserl may be understood as urging that it should be the other way round. On this interpretation, Husserl's principal objection is not so much to defining cardinal equivalence in terms of one-one mappings, but in taking cardinal equivalence as the fundamental notion. In his Chapter VII, which includes an examination of Frege's definition of the cardinality operator, he argues that the sense of an ascription of number to a set does not consist in classifying it with a group of numerically equivalent sets; we are concerned only with the set itself, and not its relation to other sets. In this, Husserl is quite right. Frege, in the celebrated example of the waiter laying plates and knives on the table in *Grundlagen*, § 70, pointed out the possibility of establishing that there are just as many things of one kind as of another without determining how many of each there are. Husserl is here pointing to the fact that it is possible to specify how many things there are of a given kind without adverting to whether or not there are just as many as things of any other kind; more exactly, that it is possible to explain what is meant by such a specification without adverting to the notion of cardinal equivalence. Both are right. It is not, indeed, possible to explain the procedure of counting save by appeal to one-one mapping: but each particular numerically definite quantifier, including 'there are denumerably many ...', may be explained without any such appeal, as Frege himself had shown in § 55 for all the finite cases. If the possibility of defining numerical equivalence without invoking definite answers to the question 'How many?' is a good argument for doing so, why is not the possibility of defining expressions of the form 'there are just n ' without invoking numerical equivalence not a good argument for

doing that? Indeed, if, in view of the conceptual priority of the latter, it is necessary to define 'the number of *F*s' in terms of 'just as many as' rather than conversely, why is it legitimate to explain 'there are two' in terms of 'the number 2' and not conversely, when the former is evidently conceptually prior to the latter? It looks as though Frege invoked conceptual priority when it suited his definitional strategy to do so, and ignored it when it did not.

The proper way to respond to Husserl's criticism, thus interpreted, is to enquire whether Frege's strategy could have been reversed. Could he have started with definitions of the numerically definite quantifiers, proceeded to define from them the corresponding cardinal numbers, and only then have defined the cardinality operator, perhaps explaining equinumerosity in terms of it? Frege strives so hard to make his actual strategy appear the only possible one that we gain some insight into the explanatory force of his definitions if in this way we explore an alternative one.

As already remarked, there is no difficulty in defining as many of the numerically definite quantifiers as we wish without invoking cardinal equivalence or the corresponding cardinal numbers. There is, for example, no need to explain 'There are denumerably many *F*s' as meaning that the *F*s can be mapped one-to-one on to the natural numbers; we may simply define it to mean that the *F*s can be enumerated.⁴ Accepting, for present purposes, Frege's identification of a cardinal number with the class of concepts having that cardinality, we could then define the number 2 as the class of concepts *F* such that there are just two *F*s, and so on for all other cases. When we need to generalise over the natural numbers, we shall need the relation of successor: this can be defined essentially in Frege's way, but with no appeal to cardinal equivalence. We should define '*n* is a successor of *m*' to mean '*n* is the class of *F*s such that, for some *x*, *x* is an *F* and the concept "an *F* other than *x*" belongs to *m*'; and we should then be able to define 'is a natural number' from the successor relation and the number 0 exactly as Frege does. With this in hand, we could with the help of a description operator define a restricted cardinality operator 'the finite number of *F*s' to mean 'that natural number *n* such that *F* is a member of *n*'; but this would of course be undefined whenever there were infinitely many *F*s.

As long as our concern is solely with the arithmetic of the natural numbers, Husserl's thesis, when interpreted in this second way, is thus vindicated. Frege differed from Dedekind in believing that the natural numbers had to be presented as finite cardinals; but it is possible to present them as the very classes of concepts with which he identified them in *Grundlagen* without invoking the relation of equinumerosity, or, therefore, the cardinality operator. It would be only at the point at which we wished to define the general operator

⁴ 'The *F*s can be enumerated' must here be taken to mean that, for some *x* and some one-one relation *R*, no *y* stands in the relation *R* to *x*, and the *F*s consist of those objects *z* to which *x* stands in the ancestral *R** of *R*.

'the (cardinal) number of *F*s' that we should be forced to define equinumerosity: Frege's thesis that the second-level relation of equinumerosity is necessarily prior in the order of explanation to that of the (unrestricted) cardinality operator cannot be gainsaid. If, following him, we wish to identify all cardinal numbers with maximal classes of equinumerous concepts, we have no way of doing so save by first saying when two concepts are to count as being equinumerous.

Frege was therefore right that, if it is the arithmetic of cardinal numbers in general that concerns us, cardinal equivalence must be treated as the fundamental notion. The sense in which this is true comes through even more clearly when we do *not* blindly follow Frege's strategy of definition, but explore a plausible alternative, one that respects relations of conceptual priority better than does Frege's own. But, equally, the sense in which it is not strictly true fails to appear when his strategy is followed: by setting the question 'What is a (cardinal) number?' at the outset of his enquiry, Frege causes us to overlook the fact that this question need not be answered in full generality if we wish only to present the natural numbers as what he took the finite cardinals to be, and hence as serving on appropriate occasions to answer questions of the form 'How many?'.

The status of the definition

We have, thus, two definitions, that of equinumerosity in terms of one-one mapping, and that of the cardinality operator in terms of equinumerosity. More exactly, we have three, the intermediate one being the rejected definition, in terms of equinumerosity, of the identity of cardinal numbers. All of these definitions raise problems concerning their status: in this chapter, we may consider only the first of the three.

If equinumerosity (cardinal equivalence) is to be a fundamental notion, then it must itself be defined if the project of deriving arithmetic from logic is to be realised; and no definition has ever been proposed save that which was already standard by the time Frege wrote *Grundlagen*. Apart from Husserl's, very few objections to it have ever been raised. Waismann criticised it as circular, on the score that it would be too restrictive to say that there are just as many *F*s as *G*s only if there actually *is* a one-one map of one on to the other: we can claim only that, whenever there are just as many of each, there *could* be such a mapping.⁵ There is, Waismann argues, no non-circular explanation of the kind of possibility involved. What is meant is that there could be a mapping *as far as the number of *F*s and of *G*s is concerned*; and thus the definition goes in a circle.

The objection is readily answered. Frege invokes no modal notions: his

⁵ F. Waismann, *Einführung in das mathematische Denken*, Vienna, second edition, 1936, pp. 77–8, English translation by Theodore J. Benac, *Introduction to Mathematical Thinking*, New York, 1951, pp. 108–9.

definition is in terms of there *being* a suitable mapping. Waismann's objection can easily be reformulated as being that Frege owed us a criterion for the existence of relations, and that no such criterion can be framed without circularity. The problem of the range of second-order quantification is indeed a serious and difficult one. For Frege a sufficient condition for the existence of a relation satisfying a given condition is the possibility of constructing a linguistic expression for such a relation, where any expression containing two argument-places for singular terms counts as standing for a relation. It needs little reflection to see that, whenever there are finitely many *F*s, and just the same number of *G*s, sufficient knowledge would in principle enable us to construct such an expression, though, when the number of *F*s was sufficiently large, this would be practically impossible. Doubts may be harboured concerning the notion of possibility in principle, which is one of intense philosophical interest and difficulty; but it is a mistake to consider it a philosopher's invention. Asked what it means to say that the population of China is about 1,008,000,000, almost anyone would say that it means that, if you were able to count the inhabitants of China, you would reach approximately that figure; the possibility here referred to is possibility in principle. In the present case, there can be no harm in invoking the notion to explain the stated condition for the existence of a relation; it would be simple to give an inductive argument to show that, whenever there were n *F*s and n *G*s, there existed a relation mapping the *F*s one-to-one on to the *G*s.

In the infinite case, however, the matter is less simple. Here the linguistic criterion is certainly inadequate: in Russell's famous example, given denumerably many pairs of socks and of shoes, we cannot specify any mapping of the socks on to the shoes, although, intuitively, there are just as many of each. The platonist characterisation of the totality of all relations over a given domain is, however, notoriously questionable. It does not follow that there is any vicious circle. When quantifiers are understood classically, the Axiom of Choice is said to hold good on a fully platonistic conception; we may, conversely, construe it as giving a criterion for the existence of functions or of sets (including relations construed as sets). One who declines to believe that there are in fact as many functions or sets as the Axiom of Choice requires may fall back on the notion of possible existence; from this standpoint, the Axiom of Choice tells us what functions or sets, and, in particular, what one-one mappings, there could be, so far as the cardinalities of other sets allow. It tells us, in other words, when there enough members of each of two sets – enough shoes and enough socks, for example – for there to be a one-one map of either on to the other; and it does so without circularity. No case is known in which two sets have, intuitively, the same cardinality, and yet the Axiom of Choice is insufficient to prove their equivalence; so we may put Waismann's objection aside as not posing a genuine difficulty.

We do much better to admit, with Husserl, the logical correctness and utility

of the criterion, and enquire into its credentials as an analytic definition. No better example of such a definition could be sought: the notion defined is one understood by all, and yet the definition is far from obvious. Certainly it is not one that occurs to everyone immediately upon being asked what 'just as many nuts as apples' means: it has already been observed that a child, who certainly knows the meaning of the phrase, is likely to answer by talking about counting the nuts and the apples. He will, however, ordinarily recognise the possibility of establishing that there are just as many of each by pairing them off, when it is pointed out to him. Can it be claimed that this definition fits the criterion of correctness given by Frege in 'Logik in der Mathematik', namely that the equivalence is obvious to everyone who understands both the defined expression and its definition?

The best way to decide whether anyone *could* fail to recognise the alleged synonymy is to enquire whether anyone *has* failed to recognise it. The answer is plainly affirmative. Husserl was one who failed to do so, and Waismann another: it is striking how faithfully their objections echo those likely to be made by an ordinary speaker. A third witness is Bolzano, who observed that the real numbers in the open interval $(0, 5)$ can be mapped one-to-one on to those in the open interval $(0, 12)$, but denied that there were just as many in the former interval as in the latter, on the ground that it was a proper subset of it.⁶

The defence may impugn the testimony of these witnesses. It may move that Bolzano's, for example, be set aside, on the ground that, regarded as an expression of everyday speech, 'just as many' has a definite sense only as denoting a relation between concepts with finite extensions, and hence that Bolzano's uncertainty was not about how it does apply to the infinite case, but about how it ought to be extended to that case. And it is true that, while the ordinary speaker would undoubtedly consider 'Infinitely many' to be an answer to the question 'How many?', he would have no inkling how to decide whether it is a definite answer like 'A hundred' or an indefinite one like 'At least a hundred'. What, then, would be the ordinary speaker's reaction to our question? Asked outright what it means to say that there are just as many *F*s as *G*s, or, as he is likely to express it, that there are the same number of *F*s as of *G*s, he will probably reply, like Husserl, that it means that, if you counted the *F*s and also the *G*s, you would get the same number. When it is pointed out to him that you can sometimes tell that the number is the same without finding out what it is, he will doubtless agree that a one-one correlation supplies a sufficient criterion for numerical equivalence, but will urge that such an equivalence may obtain even when no correlation is to be had. To this Frege's retort to Husserl may be adduced, that counting consists in setting up a correlation between the elements of a set and an initial segment of our

⁶ B. Bolzano, *Paradoxien des Unendlichen*, ed. by Dr. Prihonsky, Leipzig, 1851, §§ 20–1; English translation by D.A. Steele, *Paradoxes of the Infinite*, London, 1950.

number-words. Because we are all taught to count before we acquire any other concepts belonging to the theory of cardinality, this idea is likely to be new to the ordinary speaker. When he has considered it, he will probably make Waismann's objection, that the numerical equivalence of one set with another guarantees that their elements *could* be correlated, but not that any such correlation already *exists*. At this stage it will be necessary to convey to him the very broad sense in which the word 'correlation' is being used, and in which a correlation is being said to exist. After a good deal of explanation, he may then be brought to agree that, whenever there are the same number of *F*s as of *G*s, a one-one correlation exists between them; but he will still deny, like Husserl, that that is what he has all along meant by the statement that the number is the same.

In 'Logik in der Mathematik', Frege requires that the defining expression should have the same sense as that defined; so we must ask after his criterion for two expressions' having the same sense. About this he wavered somewhat, but his favoured criterion for the synonymy of two sentences was that anyone who understood both should recognise their extensional equivalence, i.e. coincidence of truth-value.⁷ To interpret this consistently with Frege's other views on sense, we must require the recognition of equivalence to be *immediate*, rather than consequent upon reflection; for reflection might include the recognition of a logical proof of equivalence, and to allow it would result in equating synonymy with analytic equivalence. For Frege, however, synonymy must be a much narrower relation; otherwise analytic judgements would not extend our knowledge.

It was Frege's general belief that anyone who grasps the sense of each of two expressions must know whether or not their senses coincide. In the formulation referred to above, however, he does not require that anyone who understands two synonymous sentences should recognise them *as* synonymous, only as extensionally equivalent; presumably the subject might assign distinct senses to them on the basis of some faulty conception of what constitutes the sense of a sentence. On this ground, Husserl might be dismissed as a witness for the prosecution, since, although he denied that 'There are just as many *F*s as *G*s' means 'There is a one-one map of the *F*s on to the *G*s', he agreed that, on logical grounds, they are equivalent. Can the ordinary speaker's testimony be dismissed for the same reason?

The difficulty is that he does not start with a full understanding of the defining expression; and the explanation needed to convey it to him involves a great deal that would otherwise have been the product of reflection. He is surely right to resist the contention that the explanation merely brought to light what from the outset he has understood 'just as many' as meaning: the explanation has expanded his conceptual repertoire, and therefore cannot

⁷ See, for example, 'Kurze Übersicht meiner logischen Lehren', 1906, *Nachgelassene Schriften*, p. 213, *Posthumous Writings*, p. 197.

embody only what he always consciously took the phrase to mean. The real fault lies, however, with Frege's criterion of synonymy, which commits the error of asking after the sense of the sentence in isolation from related sentences. A speaker's grasp of the sense of a sentence comprises an ability to perceive its connections with other sentences which are close neighbours in the web of language; asked, out of context, to explain the sense of a sentence, he may well overlook some of those connections, perhaps by asking himself how he could tell that the sentence was true in a given case, and omitting to ask what, knowing it to be true, he would infer from it. An analogous oversight is likely to occur if you ask someone how 'inefficient' and 'incompetent' differ in meaning: you will probably get a subtle distinction, but not the remark that the construction 'incompetent to do such-and-such' is admissible, while the parallel construction with 'inefficient' is not. Suppose a child has been taught to count, and, asked to say how many cakes there are on a plate, correctly replies, 'Twelve'. If his mother now says, 'Good: there are just enough to go round', referring to the forthcoming children's party, the child will hardly count as understanding what the statement 'There are twelve cakes on the plate' meant if he proves not to grasp the idea of their going round among twelve children. In such a case, he has merely been trained as a counting-machine, but does not understand the ascription of number which results from the operation of counting. 'There are just enough to go round' is one way of saying, 'There is a one-one correlation'; so Frege was not far astray, after all, in incorporating the notion of a one-one correlation into his analysis of ascriptions of number. As Husserl saw, we can explain what 'There are twelve . . . ' means without alluding to equinumerosity; in practice, we should not regard anyone as understanding the phrase if he failed to perceive that from 'There are twelve *F*s' and 'There are twelve *G*s' it can be inferred that we can associate the *F*s with the *G*s in such a way that just one *F* is associated with each *G*.

A speaker's understanding of an expression is thus only incompletely revealed by what he says when asked to explain it; moreover, there are no precise principles determining the distribution of the items comprised in his knowledge of the language among his component understandings of those of its distinct component expressions that are closely related to each other. For these reasons, there is often no determinate answer to be given to the question whether a proposed analytic definition does or does not capture the ordinary sense of the defined expression. Analysis makes explicit connections which we make but are not, out of context, conscious of making; it supplies, for principles to which we have been trained to conform, formulations we do not have the present vocabulary to give. An analytic definition can therefore be required to do no more than to come as close as possible to capturing the existing sense. It does so if, first, any ordinary speaker can be brought to agree that it provides a necessary and sufficient condition for the application of the expression defined; if, secondly, it appeals only to connections which we make in practice,

while someone's failure to make them would count for us as showing that he did not fully understand the expression; and if, thirdly, no rival definition is possible which has a better claim to capture the sense of that expression, as commonly understood. To this extent, analysis is necessarily reconstruction; but not to an extent justifying Frege's counsel of despair in 'Logik in der Mathematik', that we should abandon the everyday expression in favour of some newly introduced technical term.

Thus, when the criterion of the correctness of definitions is their faithfulness to our everyday understanding of the defined terms, the question whether the proposed definition of a single term is correct lacks a sharp sense. We obtain a sharper sense, however, when we ask after the correctness of a *system* of definitions of a range of interconnected terms. This is because our ordinary judgements about an individual's understanding of an expression obey no clear principle. We say that someone understands an expression, or that he fully understands it, if he knows concerning it what most speakers of the language know, and is able to use sentences containing it, and closely related sentences, as competently as do most speakers. This leaves principles governing the meanings in the language of some words as a matter for experts, of which a speaker may be ignorant without forfeiting the claim to understand those words: hence Putnam's division of linguistic labour. It also parcels out the knowledge required for a perfect mastery of the language in an uneconomical way. In a systematic account of the knowledge needed for total mastery, we should divide it without overlap into what determined the meanings of the various individual words, together with the principles governing phrase- and sentence-construction: we should not assign the same piece of knowledge to two different words, as knowledge a speaker needed to possess if he was to know the meaning of either. We should make our account as economical as possible, by not repeating the same item, which a speaker needed to know if he was to have perfect mastery of the language, as governing the meaning of more than one word or device of sentence-formation: it would be sufficient that everything one had to know for perfect knowledge of the language as a whole should, on the account we were constructing, be comprised in the knowledge determining the meaning of *some* word or form of expression. If we followed our intuitive judgements about what is required for someone to be said to understand a given expression, on the other hand, we could not conform to such a maxim of economy: we should have frequently to reckon the same piece of linguistic knowledge as determinative of the meanings of different words.

What holds good for a hypothetical account of the knowledge required for a complete mastery of a language holds good also for a system of definitions of interrelated terms such as Frege's definitions in *Grundlagen* of basic arithmetical expressions. Even when, as in Frege's case, the expressions in question are already in use, and one aim of the definitions is to be faithful to their

ordinary senses, it suffices, for that system of definitions to be correct, that it comprises everything that must be implicitly known by anyone who understands all those expressions: the system is not to be judged by how it parcels out that knowledge among the defined expressions, and hence it is no criticism that the definition of any *one* expression does not furnish everything that a speaker would ordinarily be thought to need to know to be acknowledged as understanding that expression. Particularly is this so when, as in Frege's case, the system of definitions is intended to serve as a basis for deductive proofs, and hence, in particular, when these are mathematical definitions, no matter how widely used in everyday contexts be the expressions they serve to define. Such a system of definitions serves, not merely to make explicit what is implicit in our everyday understanding, but to systematise it. This gives a further reason why analytic definitions involve *reconstruction* – which yet is in no way arbitrary. The derivation, by means of a definition belonging to such a system, of some individual statement from the laws of logic alone is therefore unreliable as an indication of the logical necessity of that statement, as ordinarily understood; but this is no defect when the purpose is like Frege's, to demonstrate the analytic character of an entire theory.

If, in devising such a system of definitions, we conform to the Bolzano/Frege maxim to prove whatever is capable of proof, we may find ourselves with some freedom to choose what to allocate to the definitions and what to the theorems immediately derivable from them: the contents of such theorems may legitimately be among the things which intuitively form part of the meanings of the terms defined. Thus, if the abstractionist account had been viable, it would have been possible to *prove* from the definitions based on it that, if two sets A and B have the same number n of members, there will be a one-one map of A on to B . For let N be the unique abstract set of n featureless units, and let ϕ and ψ be the restrictions to A and B respectively of the operation of abstraction. Then ϕ maps A one-to-one on to N , and ψ maps B one-to-one on to N : so the composition of ϕ with the inverse of ψ maps A one-to-one on to B . But this, while rendering more plausible Husserl's contention that one-one correlation is no part of the *meanings* of number-words, would not conclusively vindicate it: the content of such a theorem may well be something that must be known, at least implicitly, by anyone we should admit to have a full understanding of such words.

Frege's Definition of Cardinal Numbers

What Frege thought was wrong with the contextual definition

We may call the proposition that 'The number of F s = the number of G s' is equivalent to 'There are just as many F s as G s' the 'original equivalence'. We have seen that the supposed contextual definition of the cardinality operator, consisting of a stipulation that the original equivalence is to hold, and discussed at length by Frege in §§ 63–7 of *Grundlagen*, is in reality no definition at all. It is not, however, exactly for this reason that he rejects it in §§ 66–7, but rather because it does not determine the truth-value of every admissible sentence containing the operator. Specifically, it fails to determine the truth-value of any sentence of the form 'The number of F s = q ', when the term ' q ' is not itself of the form 'the number of G s'. Such sentences, Frege insists, cannot be set aside as inadmissible: for any two singular terms, the statement of identity connecting them must have a sense, and so any legitimate definition must provide a determinate truth-value for it. In particular, we have noted that the purported contextual definition fails to provide any means of eliminating the cardinality operator from a sentence of the form 'The number of F s = x ', where ' x ' is either a free or bound variable. If it had been a genuine definition, it would have done so; in that case, Frege's objection to it would not have arisen. His objection is, however, stronger than just that the stipulation fails to provide a method of eliminating the operator from all contexts: it faults the stipulation for failing to pass a less stringent test. We could express the test as that of determining the reference of terms of the form 'the number of F s'. A pair of recursion equations for addition or multiplication fails, within a first-order language, to provide any means of eliminating the addition or multiplication sign from all contexts; but it uniquely determines the function it denotes. Frege's objection is to the effect that the stipulation he has been considering fails uniquely to determine the mapping of concepts on to objects effected by the cardinality operator.

The context principle, presented in § 62 as furnishing the essential key to the problem how numbers are given to us, rules out as spurious all problems

about what an expression stands for that cannot be expressed within the language, or, in other words, that cannot be stated as questions about the truth-value of some sentence of the language containing that expression – about what its truth-value is, or at least how it is determined. We cannot, as it were, stand in thought outside our language, and mentally apprehend the reference of the expression; and so it is no defect of a given manner of introducing the expression into the language that it does not enable us to establish such an extra-linguistic mental association of expression and referent. Grasping the reference of an expression just is grasping certain principles governing the determination of the truth-values of sentences of the language containing it. All legitimate questions about the reference of a newly introduced term '*t*' will therefore be ones that can be framed within the language. We can legitimately ask whether the object for which '*t*' stands is of a given kind, say an organism, because this is just to ask whether the sentence '*t* is an organism' is true; and it is to be answered by appeal solely to the principles that have been laid down for determining the truth-value of such a sentence. We can legitimately ask whether '*t*' has the same reference as some other term '*s*', because this is just to ask whether the sentence '*t* is the same as *s*' is true; and the question is to be answered in the same way as the previous one. But that is all. If all those questions can be answered successfully, then the term has a reference. There is no further test it can be required to pass, such as providing us with a means of imagining its referent, or a possibility of encountering or contacting it: all nominalist objections on scores such as these spring from the vice of considering the meaning of the term in isolation.

By the same token, however, we have not fixed the reference of the term until we have supplied the means of answering all those questions of this kind that *are* formulable within the language. This involves, in particular, that, to have fixed the reference of the term, we need to have laid down determinate conditions for the reference of the newly introduced term '*t*' to coincide with that of any other given term in the language: in other words, for the truth of any identity-statement formed by putting '*t*' on one side of the sign of identity and any arbitrary term of the language on the other. It may or may not be obvious that our intention, in introducing '*t*', included its not being taken to stand for the Moon; but, unless we have provided for the falsity (or, if we wish, for the truth) of the sentence '*t* is the Moon', we cannot claim to have fixed the reference of '*t*', since we have not stipulated whether or not it stands for the Moon.

That is the fault that Frege finds with the purported contextual definition of the direction-operator. It affords us no means of determining the truth or falsity of a sentence like 'The direction of the Earth's axis is England'; and, in failing to do so, it fails to determine the references of terms for directions. And that, by implication, is the fault he finds with the purported contextual definition of the cardinality operator: it affords us no means of determining

the truth or falsity of a sentence like 'The number of planets is Julius Caesar', and thereby fails to determine the references of numerical terms. We may perhaps take it for granted that sentences of this kind are to count as false. We may urge that in practice no one will confuse the number of planets with Julius Caesar; but, as Frege remarks in § 66, that is no thanks to the attempted contextual definition. This means that we have not attained a unique specification of the reference of numerical terms formed with the cardinality operator: since we have failed to make any stipulation determining whether or not Julius Caesar is the number of planets, we have not said what the number of planets *is*, that is, what the term 'the number of planets' stands for.

Why should this matter? A first inclination might be to say that it is necessary to say what is *not* a cardinal number, as well as what is one, if we are to generalise about cardinal numbers. For example, if we merely wish to show that certain specific tasks can be effectively performed, we may rest content with laying down some merely sufficient conditions for an operation to be effective; but, if we wish to prove some proposition concerning *all* effective operations, or if, in particular, we wish to show that some task *cannot* be effectively performed, we need to have a necessary as well as sufficient condition for the effectiveness of an operation. So, likewise, it might be thought, if we wish to prove something to hold good of *all* cardinal numbers, or that there is *no* cardinal number satisfying a given condition. But this was not at all Frege's motivation. Having given his explicit definition of the cardinality operator, which supposedly overcame the problem by uniquely determining the truth-value of every sentence of the form 'The number of F s = q ', Frege uses that definition, as we have seen, solely to derive the original equivalence, and, from that, the equivalence between 'The number of F s = the number of G s' and 'There is a one-one map of the F s on to the G s'. No further appeal is ever made to the definition of the cardinality operator, because it is not needed for the proof of any proposition in which Frege is interested. To prove that there is a cardinal number satisfying a given condition, it suffices to show that there exists a concept F such that the number of F s satisfies that condition; to prove that every cardinal number satisfies some other condition, it is enough to show that, for every concept F , the number of F s satisfies that condition. That could have been done on the basis of the original equivalence, perhaps laid down as an axiom, together with the definition which Frege gives of ' n is a cardinal number', namely as meaning 'For some F , n is the number of F s'. That it is sufficient to rely on this latter definition, along with the original equivalence, of course depends on which conditions we are concerned to show some cardinal number, or all cardinal numbers, to satisfy. If Frege had wished to establish whether any cardinal number had crossed the Rubicon, he would have had to enquire whether Julius Caesar was a cardinal number; but, of course, he wished to establish only those propositions fundamental to arithmetic. For this purpose, it was sufficient to know only two things about cardinal

numbers: (1) that something is a cardinal number just in case, for some F , it is the number of F s; and (2) that the number of F s is the same as the number of G s if and only if there is a one-one map of the F s on to the G s.

Frege's ground of objection to the contextual definition he discussed at such length was thus not that he needed, in order to carry out his programme of deriving the theory of cardinal numbers from purely logical principles, to determine the truth of any sentences of the form 'The number of F s = q ', where ' q ' was not itself of the form 'the number of G s': it was simply that he considered it essential to secure a determinate reference for every expression in his theory, and, in particular, regarded it as a requirement upon a legitimate definition that it specify a unique reference for the term defined. This attitude is plainly apparent in Part I of *Grundgesetze*. A good example is his treatment of his description operator ' $\backslash$ '. This he treats formally as a first-level operator, to be attached to singular terms. The axiom governing it, Axiom VI, merely lays down that, when u is a unit class whose sole member is a , then $\backslash u$ is a . In expounding the semantics of the formal system, on the other hand, Frege is careful to provide for the case in which u is not a unit class;¹ in this case, $\backslash u$ is to be u itself. This stipulation is not needed for proving anything in the formal theory that Frege needed to prove; if it had been, it would have been incorporated into the axiom, as it could easily have been. It is not needed because Frege never attaches his description operator to any term that cannot be proved to denote a unit class; he nevertheless believed that his semantic interpretation of the formal language had to provide a reference for all well-formed definite descriptions.

Why should he have thought it necessary, or even worth while, to supply stipulations that were not to be embodied in the formal axioms, and hence never appealed to in any formal proof? The reason lies in the very purpose of carrying out proofs within a formal system. The primary advantage of the formal system is not the possibility of effectively verifying that the proofs are formally correct; it is that we can be certain that they are valid. We can be certain of this because, starting from sound fundamental laws of logic, they proceed in accordance only with valid rules of inference. We recognise that these logical laws are sound and these rules of inference valid only by seeing that the former are true and that the latter transmit truth from premisses to conclusion. If not every well-formed expression has a reference, then not every formal sentence will have a truth-value. The rules of inference will then sometimes lead from true sentences to those devoid of truth-value; since there is no guarantee that they will never allow us to derive false sentences from those that are devoid of truth-value, we shall not be able to rely on formal deductions beginning with true premisses not to arrive at false conclusions. The axioms of the formal theory could be acknowledged as true only if they

¹ Vol. I, § 11.

were expressed in a language all of whose terms possessed a reference, and hence all of its sentences a truth-value; its rules of inference could be acknowledged as valid only if they governed such a language. Frege did not aim at completeness in his formal theory; but the stipulations that lay down the references of the primitive expressions had, for him, to be complete in the sense of providing a determinate reference for every term formed by means of them, if the formal proofs were to be relied on as incontestably valid.

It is evident that *Grundlagen* leaves unfinished business to be dealt with: not just details, but a fundamental matter. The explicit definition of the cardinality operator enables us to determine that Julius Caesar is not a cardinal number only if we are able to determine that he is not the extension of a second-level concept, or, in the revised *Grundgesetze* version, of a first-level one. In *Grundgesetze*, the extension of a concept is a special kind of value-range. Unlike the notion of a cardinal number, that of a value-range is not, however, introduced by definition; if it had been, this would only have pushed the problem back one step further. The task must therefore be assigned to the semantic stipulations which provide the interpretation of the abstraction operator: they must be so framed as to determine whether Julius Caesar is a value-range, or, more generally, the truth or falsity of any identity-statement having a value-range term on one side and a term of any other kind on the other. This is precisely the problem Frege faced, and attempted to solve, in § 10 of *Grundgesetze*, Volume I. On its successful solution depended for him the legitimacy of his use of terms for value-ranges, and hence of his entire conception of logical objects and how they are given to us, and thus of the ontological status of numbers of all kinds. Russell's contradiction forced him to acknowledge that the stipulations in § 31 of Volume I did not suffice 'to secure a reference in all cases' to the terms of his symbolic language, as he admitted in his first reply to Russell of 22 June 1902. The question raised at the beginning of § 62 of *Grundlagen*, how numbers are given to us, was, by Frege's own lights, only partially answered in that book. The essential problem still remained, and could not be airily dismissed by saying, 'I assume that it is known what the extension of a concept is', as Frege tried to do in the footnote to § 68 of *Grundlagen*; on the contrary, wrestle with it as he did, he failed in the end to solve it, and his failure led to the disaster of the contradiction.

Criteria of identity

This, then, was Frege's motive for abandoning the attempt to give a contextual definition of the cardinality operator and adopting his explicit definition in terms of classes. The introduction of the notion of classes was a disastrous step, destined to bring his whole enterprise down in ruins. Frege appears to have had some inkling of its perilous nature, since in *Grundlagen* he refrains from invoking the notion save in this one place; by the time he came to write

Grundgesetze, he had unfortunately lost those qualms. According to Crispin Wright, the step was quite unnecessary, so far as the theory of cardinal numbers was concerned; if he had only realised the fact, the proposed contextual definition already resolved the Julius Caesar problem, and already determined that no cardinal number could be identified with anything not explicitly given as a cardinal number.² Frege should therefore, on his view, have contented himself with adopting the contextual definition, and thus have spared himself the disaster that overtook him. Wright does not suggest how he could have avoided appeal to classes in his theory of the real numbers. Of course, if Wright's argument is correct, Frege's explicit definition was not merely unnecessary, but actually mistaken, because it does identify cardinal numbers with objects not explicitly given as such, namely with ones given as extensions of certain second-level concepts. That definition yields the original equivalence as a consequence; but, according to Wright, if that equivalence had been adopted as a definition, or at least a stipulation, it would have ruled out Frege's definition of numbers as extensions of concepts in advance.

Frege's answer to the suggestion that the contextual definition already contains a solution to the Julius Caesar problem – or, rather, that the corresponding contextual definition of the direction-operator contains a solution to the problem of England and the Earth's axis – is given in § 67 of *Grundlagen*.

If we were to try saying: q is a direction if it is introduced by means of the definition set out above, we should be treating the way in which the object q is introduced as one of its properties, which it is not. The definition of an object does not really predicate anything of it, but only lays down the meaning of a symbol. When this has been done, it can be converted into a judgement that does treat of the object; but it now no longer introduces it, and stands on the same level as other statements about it. If one were to choose this way out, one would be assuming that an object could be given in only one way; for otherwise it would not follow from the fact that q was not introduced by means of our definition that it could not be so introduced. All statements of identity such as equations would therefore come down to saying that what is given to us in the same way should be recognised as the same. This is so self-evident and so unfruitful, however, that it is not worth while stating. In fact, we could not derive from it any conclusion that differed from all of our premisses. Rather, the significant and manifold utility of equations rests on the fact that we can recognise something as the same again although it is given in a different way.

Frege is here refusing, on general grounds, to take the easy way out by simply stipulating that no term formed by means of the direction-operator shall be taken as standing for an object denoted by a term of any other kind. His words are ambiguous between a strong thesis and a weaker one. The strong thesis is that any such stipulation would be illegitimate; and it is that thesis which he appears to need for his conclusion. As such, it appears unjustified. If, even

² C. Wright, *Frege's Conception of Numbers as Objects*, Aberdeen, 1983, pp. 113–17.

in a one-sorted formal theory, we took the direction-operator as primitive, there would be nothing to prevent us, when giving the intended interpretation of the formal language, from dividing the domain into directions and other elements, the former being denoted by direction-terms and the latter by terms of other forms. There would, indeed, be no obstacle to incorporating such a stipulation into the formal theory itself: we need only add a primitive predicate ' $D(\xi)$ ', with an axiom of the form ' $D(\text{dir}(a))$ ' and other axioms guaranteeing that ' $D(t)$ ' shall always be false when t is a term of any other form.

Even so, Frege's remarks might be defended on the ground that such a segregation of directions from all other objects could not be accomplished by anything properly called a definition. However this may be, it is not the stronger but the weaker thesis that Wright contests. The weaker thesis is that, given that the direction-operator has been *introduced* by means of the so-called contextual definition, stipulating the equivalence between ' $\text{dir}(a) = \text{dir}(b)$ ' and ' a is parallel to b ', it will still be legitimate subsequently to stipulate further that a direction-term denotes the same object as a term of another kind. Crispin Wright's belief is that the contextual means of introducing the direction-operator rules out any such identification; by contrast, Frege's argument, on the weaker interpretation, is that such an identification is left as an open possibility, and that therefore something explicit must be done if we are to exclude it. On the face of it, Frege must be right. We have seen that it is perfectly consistent with the stipulated equivalence to treat direction-terms as denoting particular lines: how can it be maintained that the stipulation in fact prohibits any such identification?

Wright's argument is that the stipulated equivalence lays down the criterion of identity – for directions or for numbers, as the case may be. Any identity-statement concerning a direction or a number must therefore be determined as true or false according to that criterion. This can happen only if the statement asserts the identity of a direction with a direction, or of a number with a number: if different criteria of identity are associated with the terms on either side of the sign of identity, there is no way in which either criterion can be applied, and hence such an identity-statement is ruled out as false without further ado. The criterion for the identity of human beings is quite different from that for the identity of numbers; and hence the stipulation specifying the latter criterion of itself determines that the statement 'The number of planets is Julius Caesar' is false.

This argument is invalidated by too simple an idea of what a criterion of identity is. The determination of the truth-value of a statement of identity between numbers may well turn on the criterion of identity for human beings; the number of Dr. Jekyll's cousins coincides with the number of Mr. Hyde's because Dr. Jekyll and Mr. Hyde are one and the same person. The oddity of Wright's position is that the legitimacy of a stipulation depends upon whether it follows or precedes another. The eccentricity of one ellipse coincides with

that of another just in case they are similar. This follows from a definition of eccentricity as the ratio between the distance between the foci and the length of the major axis; but, on Wright's view, that definition would be illegitimate if we had *first* stipulated the condition for the eccentricity of two ellipses to be the same: the geometrical criterion would preclude the identification of the eccentricity with a real number, with which is associated a quite different criterion of identity.

Frege's introduction into philosophy of the notion of a criterion of identity embodied a profound insight. This is that, whenever we speak of objects of any kind, we must have in the background a principle for determining what is to count as the *same* object of that kind. We are speaking of an object whenever we use a singular term, and also whenever it would be in place to call for a singular term, by means of a question of the form 'Which one?'. First-level quantification presupposes a domain of distinguishable objects, as do the corresponding expressions of generality in natural language: we cannot intelligibly ask whether there is anything in the room that was made in Hong Kong unless it is determinate whether or not something formerly made in Hong Kong can be identified with anything now in the room. It is part of Frege's idea that the criterion of identity for objects of a given kind is not a *consequence* of the way that kind of object is characterised, but has to be expressly stipulated as part of that characterisation. This is of course not true when the characterisation itself used the concept of a more general kind of object – a sortal concept, in the standard terminology – for which it is already determinate what is to count as the same object falling under that concept. When we introduce the concept of a prime number, we do not have to lay down when p is to count as the same prime number as q : it is already provided that it will be so counted just in case it is the same *number* as q . But as we ascend the hierarchy of ever more general kinds of object, we must reach one that was not characterised as a species of some genus; and, when the concept of an object of *this* kind was introduced, the relevant criterion of identity must have been expressly stipulated.

In §§ 63–8 of *Grundlagen*, Frege is concerned with a large class of cases in which the criterion of identity for objects of a certain kind, such as directions, shapes or numbers, consists in the obtaining of a certain equivalence relation between objects of another kind, or, in the case of numbers, between concepts. There is, however, a more basic case: if there were not, we should have no conception of objects at all, since we should not be able to appeal to an equivalence relation between objects of another kind or between concepts under which objects fall. In this more basic case, we employ what Strawson called 'feature-placing' predicates, attached, not to a proper name or other term denoting an object, but to demonstrative expressions indicating, with only rough precision, a presently observable region of space; since such predicates, when so used, are not being applied to *objects*, we may describe them as

standing for *proto-properties*. Among these feature-placing predicates are some with two argument-places, as when, using two pointing gestures, we say, 'This is darker than that'; these may be said to stand for proto-relations.

It is from this basis that we advance to the next higher level of language, at which we refer to and quantify over objects; and it is at this higher level that we have use for the first time for the concept of identity. When a learner of the language is first introduced to the notion of an object of a given primary kind – a kind not characterised as a species of some already familiar genus – the relevant criterion of identity must be conveyed to him. This will normally be done by example. The necessity for his grasping that criterion is most easily seen in the case of words such as 'letter' (in the sense of 'letter of the alphabet', not of 'epistle'), which are ambiguous between different criteria of identity: the sense in which there are twenty-six letters in the English alphabet differs in just this respect from that in which 'letter' is a six-letter word. At this first stage in the introduction of sortal concepts, and thus of a means of referring to and talking about objects, the associated criteria of identity may also be said, in a rough sense, to coincide with those for the obtaining of an equivalence relation: but, of necessity, it is only a proto-relation, not a genuine relation between objects, since we are not yet in a position to handle expressions for genuine relations. Admittedly, the criterion whereby we judge that the second letter in the word 'letter' is (in one sense) the same as the fifth might be said to coincide with that for the obtaining of an equivalence relation between letters in another sense; and the criterion for the identity of letters in that sense to coincide with that for an equivalence relation between letters in the sense in which we ask after the number of letters on a printed page; and the criterion of identity for letters in this last sense with that for an equivalence relation between what Nelson Goodman would call 'letter-inscriptions'. But this does not correspond at all to how we in fact learn to apply those criteria of identity; and, in any case, we virtually never talk or think about letter-inscriptions (though we might say, 'The letter "B" has fallen off the sign "MEMBERS ONLY"').

We may call objects whose associated criterion of identity was not explained in terms of an equivalence relation between objects of another kind (or between concepts) 'primary objects', and those whose criterion of identity is so explained 'secondary' ones. Wright is certainly correct in supposing that we should not admit any transsortal identification of primary objects, any identification of one primary object with another having a quite different criterion of identity. We have no use for any such identification, and should for an excellent reason resist it. Objects of different kinds admit different predicates: in the sense in which there are twenty-six letters in the alphabet, we may ask after the origin of the letter 'J', but it would be senseless to ask after the origin of the third letter of the word 'jejune'. If we admitted transsortal identification

of primary objects, confusion would ensue; and for this reason we do not even envisage it.

For secondary objects, the matter stands differently. They are standardly referred to by the use of an operator, like Frege's direction-operator, attached to a term for an object of the kind over which the equivalence relation is defined: the terms formed by means of this operator may be called 'fundamental terms', and the domain of the equivalence relation 'the fundamental domain'. Since secondary objects are identified by reference to a fundamental domain, they cannot be picked out by straightforward ostension. If someone points, saying, 'That book', the context may not be sufficient to dispel uncertainty about the criterion of identity he has in mind, but, this apart, there can be no unclarity about what he is referring to. If, however, he points and says, 'that shape', the ambiguity does not turn on the criterion of identity for shapes: the question may need to be asked, 'The shape of what?'. His ostension will have worked only if there happened to be a salient object in the direction of the pointing gesture. For this reason, there is not the same obstacle as with primary objects to transsortal identification.

To grasp a sortal concept covering secondary objects of a given kind *S*, two conditions must be known. First, we must know what constitutes the fundamental domain. Knowing this, we shall know to which expressions the operator may be applied in order to be sure of obtaining a fundamental term denoting an object of kind *S*; and so the circumscription of the fundamental domain may be said to constitute the criterion for the existence of objects of the kind *S*. Concerning directions, we must ask: Do all lines have directions? Is it only lines that have directions? Concerning numbers, we have to ask: Does every concept have a number belonging to it (for instance, the concept 'red', or, again, the concept 'cardinal number')? If not, what restrictions must be imposed on the predicate ' $F(\xi)$ ' if the term 'the number of *F*s' is to stand for anything? The second essential piece of knowledge we need is the criterion of identity. This will be given by some equivalence like Frege's original equivalence for the identity of numbers, or the analogous equivalence for the identity of directions; it will serve to equate the condition for the identity of the secondary objects with that for the obtaining of a certain equivalence relation between elements of the fundamental domain.

These two conditions – the criterion of existence and the criterion of identity – form the sole preparation we need for speaking of objects of the kind *S*. Of course, in order to speak of them, we need to know some predicates that can be applied to them; the introduction of such predicates is straightforward. They will be defined or explained in such a way that the two criteria comprise all the information concerning objects of the kind *S* necessary to determine their application to an object denoted by a fundamental term: a predicate so defined or explained may be called a 'fundamental predicate'. For essential purposes, this is all that is needed in order to introduce reference to secondary

objects of a given kind into the language. Nevertheless, Frege's position is sound: in the case of secondary objects, nothing stands in the way of our identifying them with objects given in some other way, provided that we respect the criterion of identity. Within natural language, we seldom make such transsortal identifications: in more systematic types of discourse, they are extremely common. Very many secondary objects may be identified with real numbers, whether within some interval such as $[0, 1]$ or otherwise: the eccentricity of an ellipse, the probability of an event, the length of a vector in Euclidean n -space, and so on. Frege, indeed, would not regard such identifications as arbitrary: since all represent a ratio of one kind or another, they conform to a common principle. A quite different kind of identification is with a representative element of the fundamental domain; this was already illustrated by the identification of the direction of a line with a line through the origin. Any such transsortal identification will make available a new range of predicates applicable to the objects so identified, in addition to the fundamental predicates. But, unlike what would happen in the case of primary objects, the possibility of applying these new predicates, which may or may not remain a possibility only in principle, will cause no confusion.

It appears at first sight as though, in rejecting the contextual 'definitions' of the direction-operator and the cardinality operator in favour of explicit ones, Frege is betraying the principle he laid down in § 62 concerning criteria of identity. That principle apparently required that the condition for the truth of such an identity-statement as 'The direction of a is the same as the direction of b ' must be stipulated outright, not derived from our prior understanding of 'is the same as', together with a definition of the direction-operator; and yet, it is in the latter way that Frege eventually proceeds. He defines 'the direction of a ' as denoting the class of lines parallel with a : from this, we are led to derive the condition for the identity of directions from that, which we already know, for the identity of classes. Specifically, it follows directly from our definition that the direction of a is the same as that of b just in case any line c is parallel to a if and only if it is parallel to b , and indirectly, in virtue of the fact that parallelism is an equivalence relation, that this will be so just in case a is parallel to b .

It is evident from the foregoing discussion, however, that the inconsistency is no more than apparent. What Frege objects to is the idea that we could define ' a is parallel to b ' as meaning 'The direction of a is the same as the direction of b '; and this, of course, he does not do, but, rather, uses the relation of parallelism to define the direction-operator. His thesis is thus not that the meaning of an identity-statement connecting two terms formed by means of the operator to be introduced must be stipulated outright, but, rather, that the equivalence relation over the fundamental domain must be already understood before the operator can be defined. That equivalence relation is to be identified by asking after the condition for the truth of such an identity-statement.

The equivalence between the identity-statement and the obtaining of the equivalence relation between the two given elements of the fundamental domain – the relevant original equivalence – will then provide a criterion for the correctness of any proposed definition of the operator: such a definition will be correct only if it yields that equivalence. The definition must provide a way of determining the truth-value of an identity-statement connecting one of the new terms with any other term whatsoever. In so doing, it may equate the reference of a term of the new kind with that of one of a different kind: all that is demanded of it is that it respect the criterion of identity embodied in the original equivalence, and that it admit the operator as defined for every argument for which we want it to be. In arriving at a definition of such an operator, the identification of the relevant criterion of identity is therefore an essential first step; very often the eventual definition will appeal to the equivalence relation in terms of which that criterion is formulated. But this is all that the doctrine of criteria of identity requires: it does *not* demand that the original equivalence incorporating the criterion of identity be itself the subject of a direct stipulation. In § 68 Frege proposes a uniform method for defining any of a whole class of operators, including the direction-operator and the cardinality operator, namely by taking it as forming terms for equivalence classes. If we assume it to be already known what a class is, as Frege expressly says that he is assuming it to be known, this is a case of equating the referent of a term of the new kind with that of one already understood, that is, of transsortal identification; it is a mistake to suppose this to involve any violation of the doctrine of criteria of identity as Frege intended it to be understood.

Frege undoubtedly took it for granted that there is nothing problematic about our referring to primary objects such as mountains, trees, people, cities and stars, or about the determinateness of reference of the proper names and other singular terms by means of which we refer to them. It is obviously true that an adequate theory of the mechanism of reference, and even an adequate exposition of the concept of a criterion of identity, must treat of primary objects; but Frege, at least in his capacity as philosopher of mathematics, ought not to be reproached for not having said enough to determine how such an account should go. The notion of a criterion of identity, as he introduced it, is applicable to singular terms for objects of every kind, as he said, which is why it is wrong to represent it, as some have done, as relating only to logical objects or to those that are not actual. It remains that, in *Grundlagen*, he was concerned directly only with its application to secondary objects, and we can hardly cavil at his restricting his discussion to them in a book about the foundations of arithmetic.

CHAPTER 14

The Status of the Definition

Logical abstraction

The passage from § 62 to § 69 of *Grundlagen* is the most important in the book. It contains the kernel of Frege's whole logicist philosophy of arithmetic, and it is there that its weakness is to be seen, as well as its strength; and therefore we are not finished with it yet. As was observed in the last chapter, the method Frege adopts for giving an explicit definition of the direction-operator, and likewise an explicit definition of the cardinality operator, namely to define directions as equivalence classes of lines and cardinal numbers as equivalence classes of concepts, is presented by him as highly general. So indeed it is, and has become a standard mathematical device used by everyone; but Frege was one of the very first to isolate it as a general device, and to perceive its wide applicability. If we wish to introduce a new type of object, but not as a subspecies of some already familiar type, and can formulate the criterion of identity for objects of this new type as the obtaining of some equivalence relation between objects of some already known kind, this method enables us to identify the new objects as equivalence classes of the old ones under that equivalence relation. Very often, the most natural way of forming terms for the new objects will be by means of an operator f to be attached to a term a for one of the objects in the fundamental domain; we shall then define this operator by setting each such new term $f(a)$ formed by means of it as standing for the equivalence class to which the referent of a belongs.

This device has since been labelled, not very happily, 'definition by abstraction'. Frege would have disliked this terminology; but he was fully aware that the device accomplished, in a legitimate way, what others attempted to accomplish by means of the operation of psychological abstraction. Both types of abstraction aim at isolating what is in common between the members of any set of objects each of which stands to each of the others in the relevant equivalence relation: Frege's logical method by identifying the common feature with the maximal set of objects so related to one another and containing the given objects; the spurious psychological operation by deleting in thought

everything except that common feature. In an important respect, it is a matter of regret that Frege hit on this device, since it prompted him for the first time to introduce classes into his logical system, and so led eventually to the catastrophe of Russell's contradiction; yet the invention of what we may call 'logical abstraction' was a highly significant contribution on his part to the logically rigorous practice of mathematics without the intrusion of appeals to psychological operations, let alone to spurious ones.

Frege was not in a position to recognise one difference, important to us, between the second-level cardinality operator and the various first-level operators to which he compares it. A definition by logical abstraction effects a partition of the domain of the equivalence relation: if the lines on the Euclidean plane form a set, then each of the equivalence classes of lines on the plane, under the relation of parallelism, is a set. But, when cardinal numbers are defined, after the manner of *Grundgesetze*, as equivalence classes of sets under the relation of equinumerosity, then, by the standards of von Neumann-Bernays set theory, every cardinal number other than 0 will be a proper class, since its union will be the universe. In standard set theory, therefore, Frege's cardinal numbers could not themselves be members of classes, and his proof of the infinity of the natural-number sequence would be blocked: that is why cardinal numbers, in standard set theory, are not defined in Frege's way, but as sets each representative of its cardinality, comprising all ordinal numbers of lower cardinality.

The status of the original equivalence

In § 64 of *Grundlagen* Frege wrote:

The judgement 'The straight line a is parallel to the straight line b ', in symbols $a \parallel b$, can be regarded as an identity-statement. If we do this, we attain the concept of a direction and say, 'The direction of the straight line a is identical with the direction of the straight line b '. We thus replace the symbol $\parallel$ by the more general symbol $=$, by distributing the content of the former symbol to a and to b . We split up the content in a way different from the original way, and thereby obtain a new concept.

This way of characterising the transition appears to commit Frege to holding that the judgeable content of the two sentences

(A1) a is parallel to b

(A2) The direction of a is the same as the direction of b

coincides: the two sentences have the very same content, or, in the terminology of his middle period, express the same thought. Clearly, this is also meant to apply to the pair with which Frege is really concerned, namely

(B1) There are just as many *F*s as *G*s

(B2) The number of *F*s is the same as the number of *G*s.

We may call this the 'synonymy thesis'. Someone who held that (A1) is just a disguised way of saying what (A2) says, and (B1) a disguised way of saying what (B2) says, and hence that (A1) should be defined as meaning (A2), and (B1) as meaning (B2), could of course cheerfully endorse the synonymy thesis: there could, on that view, be no objection to it. (A2) can undoubtedly be regarded as stating that a certain relation obtains between the lines *a* and *b*, so that it is perfectly in order to introduce an abbreviation for the expression of that relation. Likewise, (B2) can unquestionably be regarded as stating that a certain second-level relation obtains between the concepts *F* and *G*, so that it is again quite in order to introduce an abbreviation for the expression of that second-level relation. It was, however, precisely against this direction of explanation that Frege set his face: he insisted that the second member of each pair should be explained in terms of the first, and not the first in terms of the second. Is the synonymy thesis consistent with this view of the order in which it is necessary to explain them?

On the face of it, it is not. (A2) contains terms for two lines, and it is therefore uncontroversial that it may be viewed as stating a relation between lines; (B2) contains two predicates, and it is therefore uncontroversial that it may be viewed as stating a second-level relation between concepts. But (A1) contains no terms for directions, and (B1) no terms for numbers, and hence neither appears to admit an analysis as a statement of identity between directions or between numbers. The only way in which we can so construe (A1) and (B1) is by regarding them as disguised ways of expressing (A2) and (B2); and this is precisely what Frege denies.

The only alternative way of defending the synonymy thesis appears to be to maintain the opposite, namely that (A2) is not to be taken at face value, but construed as an idiomatic way of expressing (A1), and likewise for (B2) and (B1). This, however, also runs counter to Frege's evident intentions. If it were correct, there could be no objection whatever to the proposed contextual definitions: they would explain the sense of (A2) and (B2) in the most direct manner possible. But then (A2) and (B2) would not really be identity-statements at all, but merely idiomatic sentences disguised as identity-statements; and the terms for directions and for numbers that occur in them would not be genuine singular terms, but only what Frege calls in the Appendix to *Grundgesetze* sham proper names. The whole point, however, is that Frege intends them to be taken as genuine terms, standing for objects, and subject to all the logical operations, involving quantified sentences and the identity-sign, that real singular terms obey: it is just for this reason that he finds the contextual definitions wanting. This option is therefore likewise closed: there seems no consistent way in which the synonymy thesis can be maintained.

Nevertheless, other examples occur in Frege's writings. In *Grundlagen*, § 54, he says that an 'affirmation of existence is nothing other than denial of the number nought': this appears to imply that

- (C1) Dwarf elephants exist
- (C2) The number of dwarf elephants is not 0

form a similar pair. Perhaps more significantly, he says in § 57 that 'one can convert the sentence "Jupiter has four moons" into "The number of Jupiter's moons is four" '; although he does not state specifically what remains invariant under this transformation, the suggestion apparently is that the former sentence may really be construed as being a disguised form of the latter, and hence that they form yet another pair:

- (D1) Jupiter has four moons
- (D2) The number of Jupiter's moons is four.

More important than these is a remark in the lecture *Function und Begriff* of 1891. In that lecture, Frege explained the changes in his formal and philosophical logic that he had made during the silent years that separated his early from his middle period. In particular, he explained the introduction into his formal logic of the new notion of a value-range, where a value-range is to a function as a class is to a concept. The fundamental principle governing value-ranges is that which was to be embodied in the celebrated (or notorious) Axiom V of *Grundgesetze*, stating the equivalence between the generalised identity 'For every a , $f(a) = g(a)$ ' and the identity-statement 'The value-range of f is the same as that of g '. It has frequently been observed, with perfect justice, that this equivalence is formally analogous to the original equivalence between (B1) and (B2) discussed, and rejected, in *Grundlagen*, as a possible contextual definition of the cardinality operator. Frege was, of course, perfectly clear that the equivalence involving value-ranges could *not* rank as a definition of the abstraction operator used to form terms for value-ranges. Nevertheless, he says, in *Function und Begriff*, that a particular sentence stating the co-extensiveness of arithmetical functions 'expresses the same sense, but in a different way' as one stating the identity of their value-ranges (pp. 10–11). We may therefore take as our final pair

- (E1) For every a , $f(a) = g(a)$
- (E2) The value-range of f = the value-range of g .

These five pairs are not, according to Frege, entirely analogous. For the pairs (A), (B) and (E), the direction of explanation runs from (1) to (2); this is evidently true also of pair (C): and it is this fact that creates the problem

in their case. For pair (D), by contrast, Frege apparently held that the direction of explanation runs from (2) to (1): in this case, it is from the implausibility of the claim that the problem stems.

Why, then, did Frege assert the identity of content of the far from evidently synonymous members of all these pairs? One reason lies in the fact that it is a consequence of his preferred criterion for the identity of the thoughts expressed by two different sentences. This criterion is that anyone who grasps the thought expressed by the one sentence and that expressed by the other must immediately recognise either as true if he recognises the other as true; a criterion better expressed by saying that he must immediately recognise that both must have the same truth-value. As we have seen, to accord with Frege's intentions, the immediacy must be stressed: if time for devising a proof of either statement from the other were to be allowed, we should have merely a criterion for the analytic equivalence of the two sentences, which, for Frege, is a weaker relation. Frege never explicitly applies this criterion save to prove that two expressions do *not* have the same sense, as he does in *Function und Begriff* itself; but, since he claims it as a sufficient as well as necessary condition for identity of sense, it may be presumed to have influenced him in supposing that the pairs (A) to (E) consist of synonymous forms of sentence. There is no doubt that their synonymy follows from the criterion. Anyone who understands both (B1) and (B2), for example, must straightaway recognise them as equivalent. About the pair (E), our reluctance would reside in not admitting the presupposition that every function has a value-range, but this was never Frege's uncertainty: even the modification to Axiom V proposed in the Appendix to *Grundgesetze* maintains this presupposition, merely allowing that two functions will have the same value-range if they have the same value for every argument other than that common value-range. If Frege's criterion for identity of content is correct, then, modulo Frege's beliefs about value-ranges, all the pairs (A) to (E) consist of two sentences with a single content.

In 'The Limits of Intelligibility: a Post-verificationist Proposal',¹ Christopher Peacocke declares that what he calls 'Frege's Principle' is indisputable. He formulates the principle thus:

Content *p* is identical with content *q* just in case: necessarily any rational thinker judges that *p* iff he judges that *q*.

From left to right, the principle, as so formulated, is indeed indisputable: it follows immediately from the laws of identity. From right to left, it is somewhat ambiguous. Frege formulates the principle as one determining the identity of the contents of two *sentences*, and includes the condition that the thinker should grasp the content of both sentences. It is by no means indisputable that it follows from the necessity that one who does so grasp the content of both

¹ Christopher Peacocke, *Philosophical Review*, vol. XCVII, 1988, pp. 463–96; see p. 471.

should recognise them as having the same truth-value that the content of each is the same. For this, it is also necessary that anyone who grasps the content of either should thereby grasp the content of the other: not, indeed, that he should know that it is the content of the other sentence, but that he should be acquainted with the thought it expresses. This requirement makes the principle of little use in determining that two sentences are indeed synonymous; Frege surely revealed a sure instinct by employing his principle only to negative effect, to demonstrate non-synonymy.

It follows from Frege's criterion for identity of content that all five of our pairs consist of two sentences expressing the same thought. And yet this conclusion runs counter to intuition. The reason is that someone could well understand the first sentence of any pair without being in a position to understand the second. Of course, it in no way detracts from the claim that two sentences are synonymous that someone might understand one without understanding the other: he might simply not know the words. In cases (A) to (E), however, he might understand the first member of a pair without being as yet *capable* of grasping the thought expressed by the second, because he was not as yet in possession of a concept constitutive of the content of that second sentence: for instance, because he lacked the concept of a direction or of a cardinal number. That, indeed, is what Frege implies when he says that, by splitting up the content in the new way, we thereby attain a new concept. Yet this possibility is patently ruled out if the content of the one sentence is the same as that of the other: if it is the same, everyone who grasps the content of the one by that very act grasps that of the other, even if, through his ignorance of the words occurring in it, he does not know that it is the content of the other.

How is it possible to escape from this dilemma while still maintaining Frege's criterion for identity of content? One route would be by maintaining that, contrary to appearances, a grasp of the concept in question *is* required for a grasp of the content of the first member of the pair: for instance, that a grasp of the concept of a direction is required for a grasp of the content of a sentence of the form (A1) as well as for a grasp of that of one of the form (A2). But that would be implicitly to maintain that (A1) is no more than a disguised form of (A2), and hence must be explained precisely as being equivalent to (A2); and that reverses the order of explanation on which Frege so strongly insists. Besides, to hold that anyone who grasps the content of (A1) must already have the concept of a direction would contradict his thesis that it is by making the transition from (A1) to (A2) that we first acquire that concept. This way out of the dilemma therefore appears to be blocked.

The only alternative is to hold that a grasp of the concept is *not* required for a grasp of the content of the second member of the pair: that a grasp of the concept of a direction is not required for a grasp of the content of a sentence of the form (A2). But how can this be? A grasp of the concept is

plainly required for an *understanding* of a sentence of that form: it would therefore follow that more is required for the understanding of a sentence than a knowledge of its content. No hint of any such distinction is present, however, in the writings of Frege's middle period, nor, indeed, in the less systematic discussions of his early period. Only two features, not constitutive of the thought an assertoric sentence expresses, are ever allowed by him as required for an understanding of the sentence: the assertoric force attached to it, distinguishing it from an interrogative or other utterance; and the tone attaching to the words or phrasing, which evokes associated ideas or affects the hearer's expectation of what is coming next, but is irrelevant to a judgement of truth or falsity. To grasp the thought expressed by a sentence of the form (A2), we must indeed recognise that it is true or false according as the corresponding sentence of the form (A1) is true: but, if this is all that is known, the sentence (A2) may be no more than an idiomatic way of expressing (A1), its constituent singular terms only sham proper names in positions not admitting variables bound by quantifiers. Are we, then, to say that, when only this much is known, the *content* of the sentence has been grasped, although more is needed for a grasp of the thought it expresses? There is no warrant for any such distinction; but, in any case, it does no justice to the data of the problem. By treating the content of a sentence as not exhaustive of the thought it expresses, it allows that, when we have grasped the content of (A2), we have still not attained the concept of a direction; and it thus leaves unexplained how we are supposed to come by that concept by splitting up that very content in a new way.

Frege at no time proposed a distinction between the sense of a sentence and its content that would allow us to hold that, while a grasp of the sense of a sentence of, say, the form (B2) involved having the concept of a number, which a grasp of the corresponding sentence (B1) did not, still the *content* of the two sentences coincided. In *Begriffsschrift*, however, he did maintain a distinction between the content of a sentence and the way in which that content is regarded – the pattern we discern in it. One and the same design may be seen either as an array of white circles on a black background or as an array of black Maltese crosses on a white background: the design remains the same, and we perceive it as remaining the same, while we organise it now in one way, now in the other. It was precisely by adopting a new way of regarding an already given content, namely by hitting on a particular way of dissecting it into one or more variable parts and a constant part, that Frege held that we can arrive at a new concept. This, therefore, is surely the model for Frege's contention that it is by splitting up the content of (A1) or of (B1) in a new way that we attain the concept of a direction or of a number.

To grasp the content of 'Cato killed Cato', we do not have to have the concept of suicide: we have only to know the content of the name 'Cato' and what it is for a person *x* to kill a person *y*. We arrive at the concept of suicide

by considering both occurrences in the sentence of the proper name 'Cato' as simultaneously replaceable by another name, say 'Brutus', and so apprehending the pattern common to that sentence and the sentence 'Brutus killed Brutus'. Having done so, we can give a definition, laying down that '*a* committed suicide' is to be equivalent to '*a* killed *a*'. To understand the sentence 'Cato committed suicide', we have to have the concept of suicide, and implicitly to know its definition; that is, we must know that 'Cato committed suicide' is true if and only if 'Cato killed Cato' is true. The same indeed holds good for an understanding of a sentence containing a reflexive pronoun: you understand the sentence 'Cato killed himself' only if you are able, with the additional premiss 'Whoever killed Cato was a scoundrel', to deduce 'Cato was a scoundrel'.

We may refer to such expressions as reflexive pronouns as 'linguistic devices': a linguistic device, in this sense, is an expression or phrasing an understanding of which consists in the grasp of a principle whereby any sufficiently simple sentence involving it is equivalent to some other sentence not involving it. The qualification 'sufficiently simple' is required because 'A Roman senator killed himself' is not equivalent to any ordinary English sentence not containing either the reflexive pronoun or the word 'suicide': but we understand it because we know that it follows from a sentence such as 'Cato was a Roman senator and Cato killed himself', and also know 'Cato killed himself' to be equivalent to 'Cato killed Cato'. In a language employing the quantifier/variable means of expressing generality, the qualification would be unnecessary: every sentence containing a reflexive pronoun could be transformed into an equivalent one not containing it. Linguistic devices, in this sense, include definable expressions like 'suicide', an understanding of which is tantamount to knowing their definitions.

An understanding of a sentence containing a linguistic device obviously requires a knowledge of how that device works. The functioning of the device may therefore reasonably be regarded as a constituent of the sense of the sentence. So regarded, the sense of the sentence 'Cato committed suicide', or of 'Cato killed himself', is more complex than that of 'Cato killed Cato', since it requires a knowledge of the working of one or other linguistic device. This, however, conflicts with the thesis, which appears intuitively obvious and which Frege often asserted, that the sense of a defined expression coincides with that of the expression used to define it. This thesis implies that the sense of the sentence 'Cato committed suicide', equated by Frege with the thought the sentence expresses, coincides with that of 'Cato killed Cato'. Plainly, the thesis holds good only under a coarser application of the notion of sense than that according to which a grasp of the sense of a sentence containing the word 'suicide' or the word 'himself' requires an understanding of that word. Oddly enough, Frege gave no sign of ever having noticed this; but it provides a ground for distinguishing between a finer notion of sense and a coarser notion

of content. When this distinction is made, the content of a sentence may be taken as determined by what results from any transformations of it licensed by the linguistic devices it contains. Since such transformations convert both 'Cato committed suicide' and 'Cato killed himself' into 'Cato killed Cato', the *content* of all three sentences, so understood, will be the same: but the *senses* of all three will differ, because they involve distinct linguistic mechanisms. In particular, a grasp of the sense of a given sentence may require possession of a concept not required for a grasp of the sense of another sentence with the same content.

This serves to explain how Frege could have come to think that the two members of each of the pairs (A) to (E) had the same content; but it does not vindicate that contention. The analogy between our attainment of a concept like that of suicide, or that of the continuity of a function (Frege's favourite example), is a false one. The process by which we come to view 'Cato killed Cato' as saying, of Cato, that he killed himself, is explained in the *Begriffsschrift* in terms of a linguistic operation, and could be explained in no other way. If we have any conception of distinct occurrences of the content of the name 'Cato' within the judgeable content, or, in the later terminology, of the sense of the name within the thought the sentence expresses, or any conception of what it would be to replace one constituent of the content or of the thought by some other, it can only be by analogy with the linguistic expression and its components and with operations upon it: in drawing this analogy, we rely upon Frege's principle that the composition of the sentence reflects the composition of the thought. In order to grasp the content of the sentence, it is unnecessary that the possibility of any particular way of dissecting it should have occurred to us: but that possibility is intrinsic to the structure of the sentence, and Frege's explanation of this mode of concept-formation depends upon this fact.

By contrast, no similar operation on (A1) or (B1) can exhibit it in the form of an identity-statement connecting two singular terms: no singular term is to be discerned in either sentence. We can base a definition of the phrase 'committed suicide' upon our dissection of the sentence about Cato: but no definition can be framed that will effect a transformation of (A1) into (A2) or of (B1) into (B2). Precisely that was the negative conclusion of Frege's examination of the proposed contextual definition that would stipulate outright that (A2) was to be equivalent to (A1). We may justifiably speak of the transition from sentences of the form (A1) or (B1) to those of the form (A2) or (B2) as a process of concept-formation: but it is a different process from that which leads to the concept of suicide, to that of continuity, or to other concepts explicitly definable by complex predicates of first or higher level. When, as in cases (B) and (E), the transition involves the introduction of an operator of second level, it depends upon a possibly dangerous ontological assumption. Sentences of the form (B1) or (E1) make no demands upon the domain of objects over which our individual variables range. By contrast, the passage

from (B1) to (B2) demands recognition of the domain as containing at least denumerably many objects, while that from (E1) to (E2) makes an unrealisable demand upon the size of the domain, as Frege learned to his cost. Had (E2) really had no greater content than (E1), Frege's Axiom V, which states their equivalence, would have been unassailable; and then it could have given rise to no inconsistency.

In *Grundgesetze*, Frege makes no such claim: he nowhere suggests that the thoughts expressed by the two sides of Axiom V are identical. We must presume that by 1893 he had come to acknowledge to himself that the thesis which he had so vividly expressed in *Grundlagen* for the pair (A), and by implication for the pair (B), and had in 1891 extended to the fundamental pair (E), had been an aberration incompatible with his other doctrines. It clashed, in particular, with the doctrine, constantly repeated by Frege during his middle period, that the sense of part of a sentence is part of the thought expressed by the whole. This doctrine means nothing if it does not mean that a grasp of the thought depends on a grasp of that constituent sense. To grasp the thought expressed by (B2), one must have the concept of a cardinal number, or, in other words, must grasp the sense of the cardinality operator. To grasp the thought expressed by (B1), one need never have attained the concept of a number: the sense of the cardinality operator is no part of that thought. It follows that Frege's criterion for identity of content is defective: it is without doubt a necessary condition, but certainly not a sufficient one. That Frege never publicly acknowledged that he had been wrong to maintain, for these five pairs, the same content for both their members, is no proof that he did not change his mind: he was never very good at confessing past errors.

Frege's definition of numbers

Grundlagen is full of analytical definitions: definitions of expressions in common use, the apparent purpose of which is to capture the senses which they bear when they are commonly used. We have already scrutinised one of these: the definition of numerical equivalence, that is, of the binary quantifier 'There are just as many ... as ...'. In that case, doubts could be raised about whether the definition could be claimed as rendering explicit something at least implicit in any ordinary speaker's understanding of the phrase 'just as many'. There was no doubt, however, that every such speaker could be brought to recognise, at least for the finite case, the equivalence of defining and defined expressions. The definition could be said to impose greater systematisation on what an ordinary speaker grasps only hazily; but it could not be accused of importing some alien element into it. With Frege's definition of the cardinality operator, by means of which he wishes to construct all terms for cardinal numbers, the matter stands differently. He defines 'the number of *F*s' by his method of logical abstraction, and hence as 'the class of concepts *G* such that there are

just as many *F*s as *G*s'. In this case, any claim to have captured the meaning attached to phrases of the form 'the number of *F*s' by ordinary speakers of the language would be palpably unjustified. To such speakers, the very idea of a class of concepts, or even of a class of classes, would be remote; even when it had been explained to them, none would agree that the class of concepts equinumerous to the concept *F* was just what he had been intending to refer to when he spoke of the number of *F*s.

How, then, did Frege see the definition? With what right did he offer it in the course of a purported demonstration of the analyticity of arithmetical truths? It is here that we come upon a conventionalist strain in Frege's thinking. He believed that, to enable rigorous logical proofs to be given, rigorous definitions were necessary, and, further, that the common understanding of established expressions did not always fully determine how they were to be rigorously defined. A rigorous definition had, in particular, to specify uniquely the reference of the expression defined. If it was a predicate or functional expression, this required it to be defined for every object as argument; if it was a singular term, it must be assigned a unique object as referent. But common usage did not always serve to circumscribe the application of a predicate or functional expression to every possible argument; nor, for a singular term, did it always provide a criterion for whether or not the referent of that term should be identified with a given object or not. In such a case, the sense attached to the expression by ordinary speakers was defective: when it became necessary to supply a rigorous definition of it, the defect had to be remedied. The definition must respect the ordinary sense of the expression defined, to the extent that it was determinate; but, in remedying the deficiency, there was nothing to which we must hold ourselves responsible, and hence the remedy might be chosen with a view only to convenience.

Frege makes this attitude quite plain in § 69 of *Grundlagen*. He comments:

That this definition is correct will perhaps be hardly evident at first. Do we not think of the extension of a concept as something quite different?

He goes on to remark that a standard form of statement concerning the extension of a concept is that it is wider than that of another, that is, that one class includes another. After observing that no number, defined as he has defined numbers, can include any other number, he concedes that there may be a case in which the extension of the concept 'equinumerous to the concept *F*' was more or less inclusive than the extension of some other concept, which could not then be a number on his definition. He admits that 'it is not usual to call a number more inclusive or less inclusive than the extension of a concept'. But this is not for him a decisive objection: he retorts that 'there is nothing to prevent us from adopting such a way of speaking, if the case should happen to arise'.

Precisely the same attitude is displayed by Frege when, later in the book, he is discussing briefly how we ought to go about introducing numbers of other kinds – rational numbers, real numbers, complex numbers and so on. This section is principally devoted to an attack on what he here calls ‘formalism’, but would be better called ‘postulationism’: the idea that we need do no more than to lay down the laws that we want the numbers in question to obey and the conditions that we want them to satisfy, and may then assume their existence without more ado. This, he says in § 102, is to ‘proceed as if mere postulation were its own fulfilment’; it does not even establish the consistency of the postulated assumptions. Even if it did, that would not be enough to justify the appeal to an auxiliary mathematical theory to prove a proposition belonging to another. In § 101 he invokes his favourite example, the derivation of the formulae for the cosines and sines of specific multiples of θ from de Moivre’s theorem that

$$(\cos \theta + i \sin \theta)^n = \cos n\theta + i \sin n\theta.$$

Here a theorem involving complex numbers is used to prove formulae involving only real numbers. To guarantee the truth of those formulae, the mere consistency of the theory of complex numbers does not suffice: we want to know that the formulae are true, not merely that they are consistent. The example could be better chosen: the point is cogent.

How, then, does Frege believe that the complex numbers ought to be introduced? His discussion of this, in § 100, is almost light-hearted: but a serious purpose lies behind it. He directs attention, first, to the fact that we are not starting with already given meanings of the signs of addition and multiplication, as applied to the complex numbers; rather, ‘the meanings of the words “sum” and “product” are extended simultaneously with the introduction of the new numbers’. He then describes what he regards as the proper procedure as follows:

We take some object, say the Moon, and define: let the Moon multiplied by itself be -1 . We then have, in the Moon, a square root of -1 . This definition appears legitimate, since from the previous meaning of multiplication nothing follows concerning the sense of such a product, and hence, in extending this meaning, we may make arbitrary stipulations. We also need, however, the product of a real number and the square root of -1 . Let us, then, choose instead the temporal interval of one second to be a square root of -1 , and designate it by i . We can then understand $3i$ as the temporal interval of 3 seconds, etc. Which object shall we then designate by $2 + 3i$? What meaning is to be given to the plus symbol in this case?

If, in such a case, the definition had certain features due only to the choice of the one giving the definition, and owing nothing to the received sense of

the expression, how could an appeal to it be justified in demonstrating the analyticity of some statement, as ordinarily understood? The answer is obvious. If the proof of the statement does not depend essentially on the features of the definition that have been arbitrarily chosen, it will turn only on those features that were responsive to the sense of the expression as it is ordinarily used; and then the statement, as ordinarily understood, will have been adequately demonstrated. Precisely this is how it is with Frege's definition of (cardinal) number. As we already saw, the definition serves only two purposes: to fix the reference of each numerical term uniquely; and to yield the original equivalence. In deriving the arithmetical laws by means of the definition, only the original equivalence is appealed to; no other feature of the definition plays the least role. It is the original equivalence that embodies the received sense of the expression 'the number of . . .'; hence, if we grant the legitimacy of defining all terms for numbers by the use of that operator, Frege's demonstration of the analyticity of those laws is in no way impugned by the admitted partial artificiality of the definition he gives of it. Benacerraf's problem simply does not arise for Frege. He can happily assert all four of the following propositions: (i) that the laws of arithmetic can, by means of definitions, be derived by purely logical means from the fundamental laws of logic; (ii) that, in giving those definitions, we must be faithful to the received senses of arithmetical expressions; (iii) that our definitions must completely fix the identity of the natural numbers as specific objects; and (iv) that the received senses of numerical terms do not impose any one specific identification of the natural numbers.

Did Frege Refute Reductionism?

The context principle has been much discussed. What does it mean, and what role does it play in Frege's argument? Even more copious discussion has been devoted to the question whether Frege continued to maintain it in his middle or mature period from 1891 to 1906. The questions are intertwined. Either the principle did not play a crucial role in the argument of *Grundlagen*; or Frege still adhered to it in *Grundgesetze*; or the structure of his thinking about the philosophy of arithmetic underwent a radical change from one book to the other. The emphasis given to the context principle in the Introduction to *Grundlagen* makes the first of these three options highly implausible. Before asking which of the other two we should choose, we must therefore examine the significance of the principle as it figures in *Grundlagen*.

There can be no doubt, from Frege's formulation of it in § 60:

It is enough if the sentence as a whole has a sense; it is through this that its parts obtain *their* content.

that he took it, at that time, as licensing contextual definitions. If any such doubt existed, it would be dispelled by the accompanying footnote:

The problem [concerning infinitesimals] is not . . . to produce a segment bounded by two distinct points whose length is dx , but to define the sense of an equation like $df(x) = g(x) dx$.

The attempted contextual definition of the cardinality operator suggested in § 63 is not rejected because it *is* a contextual definition; that feature of it is justified in § 65. It is rejected, rather, because it fails to solve the Julius Caesar problem; and though this defect will be shared by all similar proposed contextual definitions of those operators which need in fact to be defined by means of logical abstraction (by equivalence classes), there is no suggestion that *all* contextual definitions will suffer from analogous defects. A hasty reading of §§ 62–9 would therefore prompt us to interpret the context principle as simply amounting to a justification of contextual definition. On this interpret-

ation, it is cited in § 62 simply because Frege wants to explore the possibility of defining the cardinality operator contextually, in order to show where the suggestion is at fault. Once the idea has been abandoned, and the correct solution adopted, the context principle has no positive part to play.

If we take §§ 63–9 by themselves as an enquiry into the proper definition of the cardinality operator, they will bear that interpretation. If we add § 62, however, the enquiry becomes one into the way numbers are given to us. The immediate answer provided is the context principle; it is on the strength of that principle that the enquiry is converted into one concerning the senses of sentences containing numerical terms – the first instance of the linguistic turn, as already remarked. The interpretation no longer fits very well. According to it, the context principle would not be an answer to the initial question at all, but merely a false clue, misleading us towards a mistaken answer; and there would no longer be any ground for converting the enquiry into a search for a linguistic definition. The superficiality of the proposed interpretation is confirmed if we look more widely in *Grundlagen*. To have selected the context principle in the Introduction as one of the three methodological maxims guiding the entire investigation would have been quite unwarranted if it had played only the subsidiary role which the interpretation allots to it. This is confirmed in Frege's summary of the whole course of the book's argument in §§ 106–9; in § 106, he cites it as a fundamental principle, providing the key both to the problem how numbers are given to us and to that of finding the right definition of the cardinality operator. If Frege saw it thus, he must have construed it as a guide towards the correct definition of that operator, rather than as a principle, sound in itself, tempting us to give an incorrect one.

Indeed, in § 106 he ascribes to the context principle a deeper significance yet: it is what enables us to steer between the rock of empiricism and the whirlpool of psychologism. As we saw, in the Preface to *Grundgesetze* this same role is assigned to the different principle, also stressed in *Grundlagen*, that objectivity does not entail actuality (*Wirklichkeit*). The two principles must therefore be closely connected: at least in *Grundlagen*, Frege must have seen a grasp of the context principle as an essential condition for recognising that, to possess the status of being objective, an object does not have to be actual. If we do not recognise this, we shall commit either the empiricist mistake of taking numbers to be actual, and so either physical aggregates or physical properties, or the psychologistic one of regarding them as subjective, the products of human mental processes. We cannot have a correct view of what numbers are unless we understand that they *are* objective, but that they are *not* actual: but, if we fail to grasp the context principle, we shall be unable to see how these two propositions can be true together.

Now objects which are objective but not actual are precisely what are now called abstract objects. The salient characteristic of abstract objects is taken to be that they have no causal powers; and this is essentially Frege's criterion

for an object's not being actual (*wirklich*). In virtue of this characteristic, abstract objects stand in high disfavour in certain philosophical circles. Lacking causal powers, it is said, they cannot serve to explain anything. Moreover, we can have no evidence for their existence. For, since they lack causal powers, everything would appear exactly the same to us if they did not exist. It follows that the hypothesis of their existence is completely groundless, since we can explain nothing by it and can have no evidence in favour of it: it must therefore be dispensed with.

This line of argument – which we may call ‘the nominalist challenge’ – can have no force when the existence of the abstract objects is taken to be an analytic truth, as Frege took the existence of numbers to be. An analytic truth is not required to explain anything in order to be believed: it simply defies disbelief. If it is analytically true that the natural numbers exist, we cannot intelligibly ask how things would appear to us if they did not exist: their non-existence is literally unthinkable; the question how things would be if some self-contradictory proposition held good requires no answer.

This retort does not go to the heart of the matter, however. It is not in every case an *a priori* truth that some non-actual object exists; and yet, even when it is not, the nominalist challenge is a paradigmatic example of what Wittgenstein meant by comparing philosophical perplexity to the bewilderment of a primitive confronted with a sophisticated machine. In *Grundlagen*, Frege's examples of objective but non-actual objects are the Equator and the centre of mass of the solar system. The existence of the Equator is certainly an *a posteriori* truth. It depends on the fact that the Earth has poles, which in turn depends on the unquestionably contingent fact that it spins about an axis. Yet, if someone argued that to assume the existence of the Equator explains nothing, that, moreover, since it has no causal powers, everything would be exactly the same if it did not exist, and that therefore we have no reason to accept the hypothesis of its existence, we should gape at the crudity of his misunderstanding.

What should we say to correct the objector's misunderstanding? He is trying to conceive of the Equator as an actual object that has been *stripped* of its causal powers; naturally, then, he cannot see what grounds we can have for believing in such an object. We have to teach him that it is an altogether different *kind* of object. We can do that only by patiently explaining to him the use, or the truth-conditions, of sentences containing the term ‘the Equator’; such an object as the Equator is given to us only by means of our grasp of what can meaningfully be said about it and when it is true to say it. When we have given these explanations, he will grasp that there is nothing problematic about the existence of the Equator; that its existence is not a hypothesis, but stands or falls with the proposition that the Earth rotates about an axis. Or, if he does not, we may abandon him to self-congratulation on his resistance to platonistic superstition.

The nominalist challenge was not standard in Frege's day, as it has become in ours; what corresponded to it was the classification of numbers as 'creations of the human mind' by those who, recognising that they were not constituents of the physical universe, thought that there was nothing else for them to be. Both lines of thought rest, equally, on a refusal to recognise numbers as objective but non-actual objects. We shall fail to understand Frege if we do not appreciate that, for him, such a refusal is just as crude and gross an error as taking the existence of the Equator to be a groundless hypothesis which does not succeed in explaining anything. Indeed, we shall fail to grasp the large component of truth in the context principle if we fail to acknowledge that the nominalist challenge *is* as crude a mistake as that concerning the Equator; which is not at all to say that the matter is unproblematic.

Meaning, sense and reference

There can be no doubt, from § 62, that Frege was appealing to the context principle to justify our belief in the existence of the numbers. To ask, 'How are numbers given to us?', is to ask by what means we apprehend them. It is therefore an epistemological question; but since it includes the question what entitles us to suppose that there are any such things, it is also an ontological one. Now the context principle is, in the first instance, a principle concerning meaning. It is by fixing the sense of a differential equation that the expression 'dx' obtains its content or meaning; it is by fixing the senses of sentences containing numerical terms that such terms obtain theirs. The notion of meaning or of content employed by Frege in the early period that includes the writing of *Grundlagen* was an undifferentiated one. In particular, Frege made no distinction at that time between the meaningfulness of a singular term and its denoting something: it either had a meaning or content, consisting in its standing for some object, or it was meaningless. 'The sentence "Leo Sachse is a man"' is the expression of a thought only if "Leo Sachse" designates something', as he wrote in his very early comments on Lotze's *Logik*.¹ The question whether, by fixing the senses of sentences containing numerical terms, we thereby guarantee those terms a *sense*, but not necessarily a reference, could not arise for Frege at this stage of his thinking. That is why, although he never reiterated the context principle in so many words, we cannot discuss it, as it is used in *Grundlagen*, without asking what became of it – or what should have become of it – once the distinction between sense and reference had been introduced. Should we regard it as a principle concerning sense, or concerning reference, or both? Did Frege continue to maintain it as a principle concerning sense, and is it plausible as so interpreted? Did he continue to maintain it as

¹ 'Siebzehn Kernsätze zur Logik', no. 10, *Nachgelassene Schriften*, p. 189, and *Posthumous Writings*, p. 174, where it is incorrectly printed as part of Kernsatz 9.

a principle concerning reference? If so, is it plausible when interpreted in *this* way?

As a principle concerning sense, we may take the context principle as stating that the sense of an expression relates exclusively to its role in sentences, and consists in its contribution to the thought expressed by any sentence in which it occurs. So understood, it is indisputable that Frege continued to maintain it in *Grundgesetze*. In Volume I, § 32, he wrote, 'The simple names, and those that are themselves complex, of which the name of a truth-value consists contribute to the expression of the thought, and this contribution, on the part of any one of them, is its *sense*'; by a 'name of a truth-value' Frege here means what is ordinarily called a sentence (considered apart from assertoric force), while the simple or complex names composing it are the constituent subsentential expressions. It follows that general stipulations adequate to determine the senses of a range of sentences in which a given expression occurs must suffice to fix the sense of that expression, as it occurs in those sentences; since the thought expressed by a sentence is partially constituted by the occurrence in it of a constituent with that sense, such stipulations must enable us to isolate the contribution which that expression makes to the sense of the whole. In a language adapted to the carrying out of deductive inference, as Frege was convinced that natural language is not, an expression ought, he believed, to bear the same sense in all contexts: it ought, that is, to make a *uniform* contribution to the senses of all sentences containing it. The context principle, as one relating to sense, amounts to the conceptual priority of thoughts over their constituents: the constituents *can* be grasped only as potential constituents of complete thoughts. That principle governed Frege's thinking from start to finish: however expressed, it was one to which he was constant.

What, then, does the context principle say if we interpret it as a principle concerning reference? Crispin Wright's *Frege's Conception of Numbers as Objects*² is largely devoted to this question. Both the meaning of the principle and Frege's attitude to it during his middle period are far more problematic when it is so understood than when it is regarded as relating solely to sense. For the present, we may address ourselves to the first of these two questions only, assuming, merely as a working hypothesis, that Frege continued to maintain the principle, in content if not in formulation, in *Grundgesetze*; we can reserve for later an enquiry into the degree to which the hypothesis holds good.

Wright interprets the principle as what he calls 'the thesis of the priority of syntactic over ontological categories'.³ He explains this thesis as laying down that

the question whether a particular expression is a candidate to refer to an object is entirely a matter of the sort of syntactic role which it plays in whole sentences.

² Aberdeen, 1983.

³ Op. cit., p. 51.

This is surely correct as exegesis of Frege. For him, a subsentential expression, possessing a sufficient unity to be recognised as having a reference at all, must either be 'saturated' or 'unsaturated'. An unsaturated expression is one which, to be understood, must be conceived as containing one or more argument-places; it cannot therefore be taken as referring to an object, since an object does not have arguments: if it refers to anything, it refers to a concept, relation or function. A saturated expression is a 'proper name', in Frege's terminology, or what we less misleadingly speak of as a singular term. Only such an expression can be a candidate for having an object as its referent; if it has a referent at all, that referent must be an object. Since the term that refers to it has no argument-places, the referent cannot have arguments. What has arguments is a function; an object is anything that is not a function. There can be no question of acknowledging an expression as having a referent, and then examining the referent to discern whether it is an object, a concept or a function of some other kind. If we did not know that, we did not understand the expression; and then we did not know what it was for it to have a referent.

It is true that we might query Wright's use of the term 'syntactic'. If he were referring to expressions of Frege's logical symbolism, the term would be completely accurate: in it, the distinction between 'proper names' and, for example, names of first-level functions of one argument is a syntactic one in the strictest sense. So understood, however, the syntactic priority thesis would be utterly banal: the context of Wright's discussion demands that we interpret him as referring to expressions of natural language. The question therefore arises to which principles of syntactic classification he is appealing. From the standpoint of traditional syntax, 'every actor' is a singular noun-phrase, of the same grammatical category as 'the Pole Star', but it is certainly not a candidate for referring to an object. Wright is invoking a Fregean syntax for natural language, which may not exist in its entirety, but is in any case a syntax devised with an eye to semantics. We may perhaps say that an expression plays the kind of 'syntactic role' Wright has in mind if, to use Quine's term, when 'regimenting' sentences of natural language in the notation of predicate logic, we should find it advisable to treat that expression as a singular term; or, at least, to do so when the regimentation is designed for relatively superficial purposes. Certainly this would require that the expression should conform to standard rules of inference involving quantified sentences; and certainly we could not devise tests, formulated wholly by reference to natural language, for its fulfilling that 'syntactic role' unless we were permitted to appeal to the intuitive validity of simple inferences carried out in natural language. Whether we can devise comprehensive tests involving such an appeal is a debatable matter, and one debated by Wright. His term 'syntactic role' must therefore be understood somewhat loosely; we may perhaps leave problems of syntactic classification to be dealt with as they arise, contenting ourselves with the reflection that we can in practice judge reasonably well whether or not an

expression would count for Frege as being a 'proper name', even if we cannot precisely formulate the principles underlying our judgements.

So far, so good; but, given a candidate for being an expression that refers to an object, we naturally want to know what decides whether it is to be elected or not. The question became an increasingly pressing one for Frege. In 'Über Begriff und Gegenstand' syntactical criteria induced him to deem 'the concept *horse*' to refer to an object, whereas, as he came to see, the right solution would have been to declare such phrases misbegotten; in his last years, he expressed the view that phrases of the form 'the extension of the concept *a*' are pseudo-proper names referring to nothing.⁴ Wright has an answer for this question, too, which he takes to be a further component of the context principle:

If it [a given expression] plays that sort of role [sc. the 'syntactic role' previously mentioned], then the truth of appropriate sentences in which it so features will be sufficient to confer on it an objectual reference.

Wright is of course here assuming that, by some unspecified but admissible means, we have determined the truth-conditions of sentences containing the expression. What is an 'appropriate sentence'? If *T* is the expression in question, then it might be a sentence like '*T* exists' or 'There is such a thing as *T*'. Alternatively, if we follow Frege's principle that a sentence containing an empty singular term is devoid of truth-value, it might be held sufficient that any sentence containing *T* is to be evaluated as either true or false.

The idea behind Wright's suggestion is clear. 'The Equator' is a candidate for being a proper name referring to an object because it behaves like a singular term; and it is to be admitted as in fact having an object for its referent because it satisfies the condition we have laid down for the Earth's having an

⁴ See 'Erkenntnisquellen der Mathematik und der mathematischen Naturwissenschaften', *Nachgelassene Schriften*, pp. 288–9, *Posthumous Writings*, pp. 269–70. The passage is worth quoting at length. 'A property of language that endangers the reliability of our thinking is its tendency to form proper names to which no object corresponds . . . A particularly noteworthy example of this is the formation of a proper name in accordance with the pattern "the extension of the concept *a*", e.g. "the extension of the concept *fixed star*". In virtue of the definite article, this expression seems to designate an object; but there is no object that could be so designated linguistically. From this have arisen the paradoxes of set theory, which have brought set theory to naught. I myself, in attempting a logical foundation for numbers, succumbed to this delusion, by trying to construe the numbers as sets . . . It is indeed difficult, perhaps impossible, to test every expression which language presents to us for its logical harmlessness. A great part of the philosopher's work thus consists – or at least ought to consist – in a struggle with language . . . The same expression "the extension of the concept *fixed star*"

exemplifies in yet another way the dangerous tendency of language to form pseudo-proper names. One such is

'the concept *fixed star*'

by itself. From the definite article the appearance arises that an object is designated, or, what is the same thing, that "the concept *fixed star*" is a proper name; whereas "concept *fixed star*" designates a concept and hence stands in the sharpest contrast with a proper name.'

equator, or, alternatively, because we have provided conditions for the truth of various sentences containing the term which are in fact satisfied. The condition for the Earth to have an equator is that it should rotate upon an axis, which it does. Such a sentence as 'We have crossed the Equator' is to be judged true if the speaker has made a journey at the start of which he was closer to one Pole and at the end of which he was closer to the other, and this sometimes happens. It is accordingly not within the competence of a philosopher to deny that the Earth has an equator, nor, therefore, that there is such a thing as the Equator; and we need only understand the logical category to which the expression 'the Equator' belongs to grasp that the Equator is not a concept or a relation or a function, but can only be classified as an object. This is the reasoning which Wright is advancing: advancing, rather plausibly, as reproducing Frege's thinking; and advancing on his own account as cogent.

A serious omission

Now Wright is not particularly concerned with the Equator, which, indeed, he does not mention. As the title of his book indicates, its aim is, by appeal to the context principle thus understood, to vindicate Frege's method of introducing the cardinal numbers by fixing the senses of sentences containing numerical terms. More exactly, he wishes, on the strength of Frege's context principle, to vindicate a means of fixing the senses of such sentences – and so guaranteeing a reference for numerical terms – that Frege rejects, namely the 'contextual' method discussed, largely by means of the analogy with terms for directions, in §§ 63–7 of *Grundlagen*; for Wright believes the Julius Caesar problem to have been only a spurious obstacle to that procedure. The consequence is that, for Wright, *Grundlagen* left no unfinished business. Frege's actual procedure did leave unfinished business: the cardinality operator was defined in terms of extensions of concepts, themselves archetypal logical, and hence non-actual, objects. The question then remained to be resolved how extensions of concepts are given to us, how terms for them are to be introduced; Wright's view of the matter enables him to evade this question entirely.

Yet, even if his short way with the Julius Caesar problem were sound, he had no right to ignore the question about extensions of concepts altogether. It was in Volume I of *Grundgesetze* that Frege dealt with the business left unfinished in *Grundlagen*. Extensions of concepts are introduced in the later book as special cases of value-ranges. The question therefore becomes: how are value-ranges given to us, and how are terms for value-ranges to be guaranteed a reference? We are here still proceeding on the assumption, as yet unscrutinised, that, in *Grundgesetze*, Frege still maintained the context principle, as a principle concerning reference. If so, then it must have been by appeal to it that he justified his introduction of value-ranges. This is *prima facie*

plausible, since there is an exact formal analogy between the cardinality operator and the abstraction operator. Both are term-forming operators of second level, to be attached to an expression for a concept or for a function; both are governed by a criterion of identity stated in terms of a second-level relation between the relevant concepts or functions. The abstraction operator differs from the cardinality operator, on Frege's account, in being primitive, and hence incapable of being explicitly defined. Since, by the time of writing *Grundgesetze*, Frege had repudiated the whole conception of contextual definition, he does not so much as entertain the possibility of defining the abstraction operator contextually: it is to be a primitive symbol governed by an axiom and by stipulation of its reference in the metalanguage. But this leaves it in no different case from the cardinality operator, considered as introduced in the way Wright favours, namely by the 'contextual' procedure canvassed in *Grundlagen*, §§ 63–5, and rejected in §§ 66–7. It makes no difference, for present purposes, whether this procedure is described as a 'definition' or not. Hence, if the context principle, as expounded by Wright, is enough to validate the 'contextual' method of introducing the cardinality operator, it must be enough to validate a similar means of introducing the abstraction operator.

This is why the mere fact that, on his view, it is unnecessary to define the cardinality operator in terms of classes or of value-ranges does not entitle Wright to ignore the problem of the abstraction operator. For Frege's method of introducing the abstraction operator – that is, of introducing value-ranges – was, notoriously, *not* in order. It rendered his system inconsistent; and that inconsistency forced him eventually to acknowledge that his entire enterprise had failed.⁵ If the context principle, as stated by Wright, were sound, there could have been no inconsistency. More exactly, we should distinguish between the general principle and an application of it to justify ascribing a reference to value-range terms as Frege introduces them in *Grundgesetze*. The context principle, as formulated by Wright, requires the truth-conditions of sentences containing the terms in question to have been fixed: if they have *not* been fixed, then the context principle does not genuinely apply. We have then only two options: either the context principle is not unreservedly sound; or if, in introducing the abstraction operator in the way he did, Frege took himself to be guided by the context principle, then his application of the principle was erroneous, since he failed to satisfy the conditions it lays down. It is not open to us to defend both the context principle in general and this application of it: our task must be to diagnose Frege's error and, if necessary, to delimit the scope of the context principle accordingly. But this throws the gravest doubts upon Wright's claims. We may take him as concerned to vindicate, by appeal to the context principle, a method of introducing the cardinality operator which Frege did not in fact adopt: namely, by laying down the criterion of identity

⁵ 'After the completion of the *Grundgesetze der Arithmetik* the whole structure collapsed about me' – letter to Hönigswald of 4 May 1925.

for numbers (the original equivalence), and supplementing it by some solution to the Julius Caesar problem (a stipulation governing identity-statements with a numerical term on one side and a term of another sort on the other). To all appearances, this would exactly resemble Frege's method of introducing the abstraction operator in *Grundgesetze*. In this case, we therefore have three options: to reject the context principle altogether; to maintain it, but declare that it does not vindicate the procedure Wright has in mind; and to formulate a restriction upon it that distinguishes the cardinality operator from the abstraction operator. Wright does none of these things: he maintains the context principle in full generality, understood as he interprets it, and defends the appeal to it to justify ascribing a reference to numerical terms, considered as introduced in the foregoing manner, without stopping to explain why an apparently similar manner of introducing value-range terms should have led to contradiction. He owes us such an explanation; the claim that the method of introducing the cardinality operator he envisages would obviate any use of the notion of a class supplies no excuse for his failure to provide that explanation.

Contextual definitions

For Wright, two conditions are together necessary for recognising an expression *T* as referring to an object: that *T* fulfils the 'syntactic role' of a singular term; and that we have fixed the truth-conditions of sentences containing *T* in such a way that some of them come out as true. It does not, apparently, matter by what means we fix those truth-conditions. In particular, it is plain that Wright would admit the case in which we do so by means of a genuine contextual definition, or chain of genuine contextual definitions, provided that these leave intact the syntactic role of the putative singular term *T*, as, for example, Russell's theory of descriptions fails to do for definite descriptions. It does not matter whether, in the sentence into which the definition transforms a sentence containing *T*, there is any longer a singular term corresponding to *T*: what matters is that the sentences to be transformed are so explained that, so far as the logical behaviour of those sentences is concerned, *T* is not unmasked as only a spurious singular term. This means, primarily, that the laws relating to quantifiers must remain valid. On Russell's theory, we cannot, for example, infer 'The King of France brushes his teeth at night' from 'Everyone brushes his teeth at night'; that is why, on that theory, 'the King of France' does not have the syntactic role of a singular term. We cannot accuse Wright of being unfaithful to the intentions of the author of *Grundlagen*; Frege's remarks about infinitesimals in § 60 clearly show that he is not.

Wright makes clear the admissibility, from *his* standpoint, of contextual definitions by choosing, as his central example, precisely the introduction of terms for directions, as discussed by Frege in §§ 63–5. He considers the

truth-conditions of sentences about directions as determined by three sets of stipulations:

- (1) the identity condition that the direction of a is the same as the direction of b if and only if a is parallel to b ;
- (2) a series of stipulations to the effect that a predicate A_i is to hold good of the direction of a just in case some known predicate F_i holds good of a , where parallelism is a congruence relation with respect to F_i ;
- (3) for each such A_i , the stipulation that 'For some d , $A_i(d)$ ' is to be true if and only if 'For some a , F_i (the direction of a)' is true.

There is a little uncertainty over whether these stipulations are intended to govern a two-sorted or a one-sorted language. In Wright's first formulation of them, he speaks of 'quantification over directions', and uses ' d ' as a bound variable ranging over directions, and ' a ' as one ranging over lines.⁶ When the stipulations are later repeated, ' a ' is still used as both a free and a bound variable over lines, but ' x ' as a bound variable over directions and also a free variable over lines.⁷ The point is not, however, of crucial importance; Wright makes plain that he is wishing to consider a set of stipulations together constituting a complete contextual definition, enabling sentences involving reference to or quantification over directions to be transformed into ones relating only to lines.

We could also consider an explanation of numerical terms, and of arithmetical sentences containing them, by means of the interpretation of numbers as concepts of second level, in line with Frege's abortive definitions in § 55 of *Grundlagen*. Wright's objection to this is the standard one, that it would make the infinity of the sequence of natural numbers depend on there being infinitely many objects other than numbers (or classes); but this does not affect the present point. If we so interpreted arithmetical statements, we should have to agree that how many cardinal numbers there were depended upon how many non-logical objects there were: the question presently at issue is whether our so interpreting them would be compatible with our maintaining that those cardinal numbers which did exist were objects. The interpretation is naturally described as embodying a refusal to take numerical terms at face value, as being genuine singular terms, and that is how it was described when it was discussed in an earlier chapter. If the context principle, as Wright understands it, is correct, however, the description is tendentious: we could explain sentences containing numerical terms in exactly the same way – by transforming

⁶ C. Wright, *Frege's Conception of Numbers as Objects*, pp. 29–30.

⁷ *Ibid.*, p. 67.

them into sentences involving numerically definite quantifiers – but still insist that those terms are genuine singular terms standing for non-actual objects, the numbers.

Wright distinguishes three ways of regarding such a system of contextual definitions: an austere way, a robust way and an intermediate way. The austere interpretation is that of an intolerant reductionist. Such a reductionist claims that to explain sentences about directions by translating them into sentences about lines, or sentences about numbers by translating them into ones involving expressions for and quantification over second-level concepts, commits us to denying that there are any such objects as directions or numbers. Such an explanation, according to him, precludes discernment of any genuine semantic structure in the sentences so explained; they merely possess misleading surface forms.

The robust interpretation, which Wright favours, is that the contextual definitions succeed in conferring upon sentences containing the terms contextually defined senses which warrant our viewing them as having just that semantic structure which their surface forms suggest. The defined terms are genuine singular terms, with a genuine reference, albeit to abstract objects. The intermediate interpretation is attributed by Wright to me, on the basis of what I wrote in my *Frege: Philosophy of Language*. Ultimately, Wright fails to find this intermediate view coherent: he doubts if there is any tenable position between the austere and robust interpretations. As concerns contextual definitions, properly so called, I shall here maintain an intermediate view, perhaps one more austere than that which Wright had in mind. I shall, however, spend no time in discussing either how faithfully Wright represents the views I expressed in *Frege: Philosophy of Language*, or how far those I advance here diverge from them.

The intermediate interpretation, as I here understand it, is that of a *tolerant* reductionist. He holds that the contextual definition serves to explain what it means to say, 'There is a direction orthogonal to those of lines *a* and *b*', or 'There is a prime that divides both 943 and 1357', rather than to show that we ought not to say things of that kind. He therefore agrees that it would be wrong to say that neither directions nor numbers exist, even if we adopt the contextual definitions: you cannot consistently combine the assertion that there is a number satisfying a certain condition with the declaration that there are no numbers whatever. He recognises further that '“31” refers to an object' can be construed untendentiously as simply the equivalent, in the formal mode, of 'There is such a number as 31', and hence as uncontroversially true. What he denies, however, is that the notion of reference, as so used, is to be understood realistically.

What does this denial amount to? The difference between Frege's early period, during which *Grundlagen* was written, and the middle period which extends over the composition of both volumes of *Grundgesetze* is not merely

that, in the latter, he distinguished between sense and reference within the former inchoate conception of content or meaning. Certainly his failure, during the early period, to draw any overt distinction between the significance of an expression and what it signifies left an incoherence in his thinking which he rectified only when he drew the sense/reference distinction: that is why it is so grievous an error to attempt to extract from the writing of his early period a system of logical doctrines as articulated and consistent as that expounded in the middle period. There is, however, a deeper difference. In a clear sense, Frege did not even aim, in his early period, at constructing a philosophical *theory* of logic. The context principle, as enunciated in *Grundlagen*, can be interpreted as saying that questions about the meaning (*Bedeutung*) of a term or class of terms are, when legitimate, internal to the language. We know the meaning of a term, say 'the Equator', when we know the conditions for the truth of any sentence containing it; that is all we need to know, and all we can know. Hence, to determine the meaning of a term, what we have to do, and all that we have to do, is to fix the senses of sentences in which it occurs. Reference therefore does not consist in a mental association between the term and the object, considered as apprehended by the mind independently of language; nor can it consist, we may add, in the existence of a causal chain leading from the object to an utterance of the term. It follows that any legitimate question about the meaning of a term, that is, about what we should call its reference, must be reducible to a question about the truth or otherwise of some sentence of the language. To ask whether a term '*a*' denotes something with spatial location is to ask whether the sentence '*a* is somewhere' is true; to ask whether '*a*' and '*b*' have the same reference is to ask whether '*a* = *b*' is true; to ask whether '*a*' has a reference is to ask whether 'There is such a thing as *a*' is true. Questions about the meaning or reference of a term that cannot be thus formulated in the material mode are illegitimate and derive from attempting to ask after its meaning in isolation. In particular, there can be no further specifically philosophical enquiry needed, beyond the relevant enquiry within the subject-matter to which the term relates, in order to establish whether or not it stands for anything.

All this accords very well with Wright's account, which we may therefore recognise as in large degree a faithful exegesis of Frege's use of the context principle in *Grundlagen*. This the proponent of the intermediate interpretation acknowledges by allowing that, so understood, the claim of a term to have a reference is not impugned by its having been introduced by a contextual definition. Viewed against the background of Frege's middle period, however, the whole framework of the discussion is altered. Now the notion of *Bedeutung* incorporates a whole theory of how the truth-value of a sentence is determined in accordance with its composition, while the notion of sense serves to sketch how a theory of what we know when we understand an expression or a sentence

may be made to rest upon the theory of *Bedeutung* as a base, the sense being the way in which the *Bedeutung* is given to us. In *Grundlagen*, no such theory is envisaged. Frege was certainly interested there in the semantic analysis of particular forms of sentence, as in the discussion of ascriptions of number (*Zahlangaben*) in § 46 or of 'All whales are mammals' in § 47; but he had no apparatus that would supply the resources for any general theory. In *Grundlagen*, the semantic discussion is treated as being on the same level as the sentences on which it bears. That is why Frege so unconcernedly employs his jargon, speaking of the number 4's belonging to the concept *moon of Jupiter* instead of speaking of there being four moons of Jupiter: expressions which we should think of as having their home in the metalanguage are not kept segregated from expressions of the object-language, but substituted for them or jumbled together with them, because Frege has no distinction between metalanguage and object-language in mind.

In *Grundgesetze*, all is different. There there is the sharpest distinction between the object-language, which in this case is Frege's formal language, and the metalinguistic stipulations, stated in German, of the intended references of expressions of the formal language. Now the conception of sense and reference with which Frege operated throughout his middle period was as follows. The sense to be attributed to an expression depends on what is involved in grasping thoughts expressed by sentences containing it. To grasp the thought expressed by a sentence is to know what determines it as true or as false. The references of the component expressions constitute their respective contributions to the determination of its truth-value; and the sense of any one of them constitutes the particular way in which its reference is given to one who grasps the thought. Our conception of the way its truth-value is determined is therefore itself articulated, in a manner corresponding to the articulation of the thought and of the sentence expressing it: we have a particular way of conceiving of a certain object as being picked out by each singular term, a particular way of conceiving of a relation as obtaining or failing to obtain between any two given objects, and so on, which jointly yield for us a particular manner in which one or other truth-value is arrived at.

When the sense of a term is given to us by means of a contextual definition, however, this model ceases to apply. Our grasp of the thought expressed by a sentence containing the term is mediated by our knowledge (possibly only implicit) of how to arrive at an equivalent sentence not containing that term. The notion of the reference of the term, as determined by its sense, plays no role in our conception of what determines the thought as true or false, nor, therefore, in our grasp of the thought; the attribution of reference to the term may be defensible, when tolerantly viewed, but is semantically idle.

Wright strives valiantly to resist this conclusion, and, more stoutly yet, to resist the austere interpretation. Again, we cannot say, on behalf of the Frege of *Grundlagen*, that he is wrong. At the end of his review, published in 1885,

of Hermann Cohen's book on infinitesimals,⁸ Frege wrote:

As regards the foundation of the differential calculus, we must, in my opinion, go back to the concept of a limit as understood in analysis, which, owing to his misunderstanding of it, the author disparages as a 'negative' one. I recently indicated, in my *Grundlagen der Arithmetik* (p. 72, fn. 1), how by means of such a foundation it is possible to secure for the differential a certain self-subsistence [*Selbständigkeit*].

If we disregard non-standard analysis, virtually everyone would construe an explanation of differentiation by appeal to limits in a reductionist spirit, as showing that we need not understand differential equations as involving any reference to infinitesimals; Frege, on the contrary, appears to think that, in the light of the context principle, such an explanation vindicates the conception of 'dx' as denoting an infinitesimal quantity. Yet we are exploring a region of Frege's thinking which we know in advance cannot be rendered fully coherent, since otherwise he would not have fallen into inconsistency; it is this feature of the topic which Wright appears persistently to forget.

He opposes the austere interpretation by standing the usual argument for it on its head. He proposes that it is better to regard a sentence ostensibly only about lines, but equivalent by contextual definition to one about a direction, as having a misleading grammatical form, than so to regard the one about directions, as the reductionist does: the former sentence, he claims, 'achieves a reference to a direction without containing any particular part which so refers'.⁹ This appears to fly in the face of Frege's insistence that it is through our prior understanding of propositions stating that lines are parallel that we attain the concept of a direction. If that is so, then we can understand such a proposition before we have that concept; and how could we understand it if it involved a reference to something of which we as yet had no conception? Wright's answer is that, while the statement about lines has epistemological priority, that about directions has 'ontological priority':¹⁰ but what can this mean? If, indeed, two sentences have the very same sense, and one involves reference to a direction, the other must do so as well: genuine reference to an object must be an intrinsic feature of the sense, rather than characterising merely the manner of its expression. We saw earlier, however, that Frege was wrong, on his own principles, to hold that a definitional transformation leaves sense wholly unaltered. This cannot be true if sense incorporates everything concerning the linguistic expression that goes to determine its reference. The alternative is to deny that the sense simply is the way in which the referent is given, by admitting different ways in which one and the same sense can be expressed

⁸ H. Cohen, *Das Prinzip der Infinitesimal-Methode und seine Geschichte*, Berlin, 1883.

⁹ C. Wright, op. cit., p. 32.

¹⁰ Ibid., p. 31.

and in which it may be grasped. When these different ways of grasping a single sense are allowed to vary to the extent that one involves apprehending that an object of a certain sort is being referred to, while the other is compatible with ignorance of the very concept of objects of that sort, the link that exists in Frege's theory between sense and reference has been snapped; and now it is quite unclear what kind of thing the sense of an expression is at all. The claim of 'ontological priority' for sentences containing the contextually defined terms divorces the notion of reference from that of sense, just as the proponent of the intermediate interpretation maintained. If someone can understand the statement about lines without being aware that it involves any reference to a direction, he has a conception of what would determine it as true into which reference to the direction does not enter. That means that the attribution of such reference is semantically idle, precisely in accordance with the intermediate interpretation.

All this, however, is seeing the matter from the perspective of *Grundgesetze*, when a semantic theory is in place, informed by the distinction between sense and reference. The perspective of *Grundlagen* differs altogether. The doctrine of *Grundlagen* is, in effect, that there is no metalinguistic standpoint: such would-be metalinguistic statements as 'The term "*a*" refers to an object' reduce to, and can only be understood as, statements such as 'There is such a thing as *a*' which belong to the language itself, a language which we cannot allude to as the 'object-language', because no other language is under consideration. Thus, in *Grundlagen*, the context principle amounts to a repudiation of the possibility of a semantic theory explaining, as from outside the language, the mechanism by which its sentences are determined as true or as false.¹¹ The context principle, as understood in *Grundlagen*, therefore admits only a thin notion of reference, that notion according to which "The direction of *a*" refers to something' is indisputably true, because it reduces to 'The line *a* has a direction', and "The direction of *a*" refers to the direction of *a*' trivially true, because it reduces to 'The direction of *a* is the direction of *a*'. The context principle of *Grundlagen* is thus strictly analogous to the redundancy theory of truth, that theory which admits only the thin notion of truth according to which 'Cleanliness is next to godliness' is true' reduces to 'Cleanliness

¹¹ I mean, of course, 'repudiation of the possibility of a semantic theory altogether, such a theory being one that would explain . . .', and not 'repudiation of the possibility that a semantic theory would be able to explain . . .'; if I had meant the latter, I should have written 'the possibility of a semantic theory's explaining . . .'. A decade or so ago, the risk that my sentence would be misunderstood would not have occurred to me; but now that few writers or publishers evince a grasp of the distinction between a gerund and a participle, so that phrases like 'due to him visiting Rome' constantly appear in print, it has become substantial. I preferred, however, leaving the sentence as I first wrote it, while adding this footnote, to resorting to inelegant periphrasis. People frequently remark that they see no point in observing grammatical rules, so long as they convey their meaning. This is like saying that there is nothing wrong with using a razor blade to cut string, so long as the string is cut; by violating the rules, they make it difficult for others to express *their* meaning without ambiguity.

is next to godliness'. It was on the basis of the context principle, so understood, that Frege, who at that time allowed contextual definitions as legitimate, rejected an austere view of them, as his remarks about differentiation demonstrate. From the perspective of *Grundlagen*, there is no more substantial notion of reference, and hence there is no room for an intermediate view, which cannot even be stated in the terms allowable by the *Grundlagen* doctrine: this is what prompts Wright to judge it to be incoherent. Are we then to credit Wright with giving a faithful interpretation of the Frege of the *Grundlagen* when he attributes to him a robust way of construing all legitimate means of introducing new terms into the language, including contextual definition?

The answer depends on what 'robust' means. If it means simply 'not austere', so that a robust view is merely the negation of the view that a reference may not be ascribable to the new terms at all, then Wright's interpretation is indeed faithful to *Grundlagen*. But, when we recall that his formulation of the context principle involved the ascription of an 'objectual reference', we may doubt this mild reading of the word 'robust'. From the standpoint of *Grundgesetze*, there is a more substantial notion of reference than the thin one allowed by *Grundlagen*, that notion, namely, employed in the semantic theory: to ask whether an expression has a reference in this sense is to ask whether the semantic theory assigns one to it, or needs to do so, where reference is a theoretical notion of that theory. Wright's notion of objectual reference appears to be just such a substantial notion, at home in *Grundgesetze* but not in *Grundlagen*. This is not, of course, to criticise Wright for adopting a semantic viewpoint, which he is entirely justified in doing; but one cannot combine this with embracing the whole doctrine of *Grundlagen*.

Wright argues further that the austere view 'is not even an option' when the contextual definition does not serve to *introduce* new expressions into the language, but to explain existing ones.¹² This depends, however, on whether the contextual definition is put forward as giving the senses we already attach to sentences containing the expressions defined, or merely to show how we *could* explain them without disturbing our existing use of them. A thoroughgoing reductionist will indeed claim that the mere *possibility* of contextually defining the direction-operator shows that there are no such things as directions; but we may cheerfully reject even the intermediate view of the matter, as here formulated, if the contextual definition fails to give the senses we actually attach to sentences about directions, for then we have no reason to suppose the notion of reference to be semantically idle. In that case, however, it must be possible to explain what those actual senses are, in some manner that accords to terms for directions a reference that is *not* semantically idle: we cannot, as Wright thinks, simply brush the question aside on the ground that the word 'direction' was already in use in the language before any philo-

¹² Ibid., p. 68.

sopher or geometer busied himself with it. Whether, and, if so, how, it is possible to explain abstract terms otherwise than by contextual definition, and what substance the notion of reference has, when applied to them, are questions deferred to the subsequent chapters.

Frege's realism about mathematics was already in place in *Grundlagen*: the famous ringing declaration in § 96 that 'the mathematician . . . can only discover what is there, and give it a name' leaves us in no doubt about that. Yet we may well feel dubious whether he was entitled to his realism. What the rhetoric means may be glimpsed by contrasting empirical with mathematical concepts. To make an empirical concept, say *comet*, sharp, we need a criterion for whether an object given directly or indirectly by observation falls under it, and a criterion for whether an object falling under it and given by a certain observation is the same as an object falling under it and given by some other observation. A realistic conception of the external world assures us that, once we are satisfied that the concept is sharp in these respects, we need do no more to guarantee determinate truth-values for quantified statements involving it, statements to the effect that there is a comet satisfying some condition, or that all comets satisfy some other condition. In general, the determination of the truth-values of our sentences is effected jointly by our attaching particular senses to them and by the way things are. We do not need to specify what comets there are, once we have rendered our concept of a comet sharp: reality does that for us, and reality therefore determines the truth or falsity of our quantified statements. So, at least, realism assures us.

Hardly anyone is realist enough about mathematics to think in the same way about quantified mathematical statements. A fundamental mathematical concept, say *real number*, which determines the domain of quantification of a mathematical theory, must indeed have a criterion of application and a criterion of identity. Given a mathematical object, specified in some legitimate way, we must know what has to hold good of it for it to be a real number; and, given two such specifications, we must know the condition for them to pick out the same real number. Few suppose, however, that, once these two criteria have been fixed, statements involving quantification over real numbers have thereby all been rendered determinately true or false; to achieve that, it would be generally agreed that further specifications on our part were required, in some fashion circumscribing the totality of real numbers and laying down what real numbers there are to be taken to be. Frege was, perhaps, an exception to this generalisation. For certainly, as we shall see, his stipulations concerning the value-ranges comprised by the domain of the formal theory of *Grundgesetze* go no further than supplying criteria for something's being a value-range and for the identity of value ranges; nothing resembling a circumscription of the domain occurs. Of course, since value-ranges are logical objects, the truth-values of quantified statements of the theory would not then be determined by what value-ranges there happened to be, as those of quantified statements

about comets are determined by what comets there happen to be, but by what value-ranges there *must* be.

This is no more than speculation about how Frege thought, for the sake of attaching some non-metaphorical content to his rhetoric. If it is correct, Frege was profoundly mistaken; so strongly realistic an interpretation of mathematical statements cannot be sustained. But, whatever be the right understanding of the realism of *Grundlagen*, on what does it rest? Only on ascribing to mathematical terms the thin notion of reference that is all that *Grundlagen* admits. The proponent of the intermediate view of terms introduced by contextual definition – the view for which I have here argued – maintains that that thin notion of reference will not bear the weight of a realistic interpretation of those terms; and, since *Grundlagen* does not allow a more substantial notion of reference to be ascribed to any terms, however introduced, there can be no basis for realism about mathematics, or, indeed, about anything else. Within the framework of *Grundlagen*, it cannot be asked whether the notion is semantically idle when applied to contextually defined terms, because it is not semantically operative, whatever it be applied to.

Realism is a metaphysical doctrine; but it stands or falls with the viability of a corresponding semantic theory.¹³ There is no general semantic theory in, or underlying, *Grundlagen*; the context principle repudiates semantics. That principle, as understood in *Grundlagen*, ought therefore not to be invoked as underpinning realism, but seen as dismissing the issue as spurious. There is a semantic theory in *Grundgesetze*; and it is in the light of that theory that we must assess the conclusions drawn by Wright from the context principle. Admittedly, we have so far advanced very little distance, and are not yet in a position to pronounce on abstract terms in general from the standpoint of a theory of reference like that of *Grundgesetze*. We can, however, surely conclude that, where the notion of reference is semantically idle, it cannot be appealed to in justification of realism. Full-fledged realism depends on – indeed, may be identified with – an undiluted application to sentences of the relevant kind of a straightforward two-valued classical semantics:¹⁴ a Fregean semantics, in fact. This excludes an explanation of certain terms by a rule for transforming sentences containing them into equivalent sentences containing neither them nor any corresponding terms, which is what a contextual definition will yield. It is on contextually defined terms that Wright chose to take his stand; concerning them, at least, we must judge that his case fails.

Wright's further arguments relate, not specifically to contextually defined terms, but to terms for abstract objects generally; consideration of them is

¹³ I have argued in various places that the only route to a vindication or refutation of realism must go through a meaning-theoretic enquiry into the right form of semantic theory; the argument is given in the greatest detail in M. Dummett, *The Logical Basis of Metaphysics*, Cambridge, Massachusetts, and London, 1991.

¹⁴ See M. Dummett, 'Realism', *Synthese*, Vol. 52 (1982), pp. 55–112.

therefore postponed. His courage in tackling the most difficult case for his view is to be admired; but he appears to regard it as the typical case, which it surely is not. Certainly the cases that concern us in considering Frege's philosophy of arithmetic – those of cardinal numbers and of value-ranges – are not instances of contextual definition. Wright makes a good case for thinking that, at the time of writing *Grundlagen*, Frege would have been willing to ascribe a reference to contextually defined terms on the strength of the context principle. He errs in supposing that this would have been the substantial notion of reference used in *Grundgesetze*, rather than the thin one that is all the context principle of *Grundlagen* allows. In the absence of a semantic theory, or of any desire for one, we cannot even say what it is to put a realistic interpretation upon any given range of sentences. Once Frege had such a theory, he disallowed contextual definition altogether. Even if this be waived, we are bound, in the presence of such a theory, to acknowledge that the robust view is untenable for terms for which contextual definition is the only way in which to explain them, or the one most faithful to our ordinary understanding of them: the intermediate view is the closest we can come to accommodating Wright's claims.

CHAPTER 16

The Context Principle

The role of the context principle in *Grundlagen*

On the face of it, the explanation of an expression – in our case, the cardinality operator – by means of an explicit definition renders otiose an appeal to the context principle to justify ascribing a reference to it. This claim does not depend on interpreting the context principle as doing no more than declare legitimate explanations by means of contextual definition. It does not matter whether the alternative was a contextual definition, or a series of them, or some other form of definition, or yet an explanation not amounting to a definition. Whatever the alternative, it appears that, when we can define a term explicitly, we do not need to appeal to the context principle, or any other, to warrant the ascription to it of a reference, providing, of course, that we acknowledge the definiens as having a reference. It therefore appears perplexing that, having eventually arrived at an explicit definition of the cardinality operator, Frege should, in § 106, emphasise the context principle as an essential step on the route to that definition.

A bad explanation would be that the cardinality operator is defined in terms of extensions of concepts, that extensions of concepts are in turn to be explained, in *Grundgesetze*, as forming a special kind of value-range, and that the ascription of reference to terms for value-ranges can be justified only by appeal to the context principle. In *Grundlagen*, Frege is plainly not in the least occupied with the question how to justify ascribing a reference to terms for extensions of concepts: he is simply taking the notion of the extension of a concept for granted as unproblematic. His citation of the context principle in § 106 does not read like a glance ahead to the completion of the unfinished business: it is meant to remind the reader of an indispensable step in the preceding argument.

The resolution of the perplexity is not far to seek: it resembles the resolution of that concerning Frege's appeal to the criterion of identity for directions. Frege appeared to claim that we could not derive, from a knowledge of what the direction of a line is, the condition for two lines to have the same direction:

he then apparently proceeded to derive that condition from an explicit definition of the direction-operator. But this appearance was illusory. What he was claiming was that we could not, from a prior understanding of what directions are, arrive at a grasp of the concept of lines' being parallel. The criterion of identity, stated in terms of that relation, was not itself a definition, or part of a definition, of the direction-operator, but a condition for the correctness of a definition of it: such a definition could be correct only if the criterion were derivable from it. Something very similar holds good in the present case. The flaw in the foregoing argument that the context principle is not needed to justify ascribing a reference to an explicitly defined term lies in its neglect of what is required to recognise a proposed explicit definition as correct – in our case, a definition of the cardinality operator. If we supposed that an assignment of a reference to a term consisted in a mental association of the term with the referent, apprehended independently of language, we should never allow Frege's definition of cardinal numbers as extensions of concepts. We do not directly apprehend extensions of concepts, and certainly not extensions of second-level concepts, or of concepts under which extensions of other concepts fall. Once we have grasped the context principle, however, we recognise that this is quite the wrong way to think about the matter. What is needed is a definition that will fix the truth-conditions of sentences in which numerical terms occur. It may be recognised as correct provided (a) that it confers determinate truth-conditions on every admissible such sentence, and (b) that it confers the *right* truth-conditions on those of such sentences for which there are 'right' truth-conditions. Particular truth-conditions may be considered right for a sentence containing numerical terms if they are those required by the sense we ordinarily attach to that sentence, supposing that we do ordinarily attach a sense to it. It so happens that, provided that we give suitable definitions for other arithmetical expressions, condition (b) can be fulfilled as long as the cardinality operator is so defined as to satisfy the original equivalence

The number of *F*s = the number of *G*s
if and only if
there are just as many *F*s as *G*s.

Moreover, the senses we ordinarily attach to sentences containing numerical terms do not suffice to determine the references of those terms uniquely. Hence, while condition (a) demands that our definition should determine them uniquely, we are at liberty to do so in any manner that does not violate condition (b). Thus, as before, the derivability of the original equivalence – the criterion of identity for numbers – becomes a condition for the correctness of a definition of the cardinality operator. What the context principle teaches us is to be *satisfied* with a definition from which the original equivalence can

be derived, or, more exactly, with any definition fulfilling our two conditions. If we do not acknowledge the context principle, we are certain to reject a definition of the cardinality operator such as Frege gives. Having understood the principle, we shall realise that a definition of such a kind accomplishes everything that a definition of numerical terms can be required to do or can by any means achieve.

A compositional interpretation of the context principle

To grasp the sense of an expression is to apprehend the contribution that it makes to the thought expressed by any sentence in which it occurs. But what is it to know this? Must we understand every sentence in which the expression occurs? Obviously not: for the understanding of such sentences will depend on our grasping the senses of other expressions occurring in them. But suppose we *do* understand all such sentences: does our understanding of them *constitute* our understanding of the given expression? Again, obviously not. Our understanding of them – indeed, if the expression has a uniform sense, of any one of them – is a sufficient condition for our grasping the sense of that expression, since one cannot grasp the thought expressed by a sentence unless one grasps the senses of all its constituent expressions. But we grasp the sense of a sentence *by* knowing the senses of its constituent expressions: it is because we already know those senses that we are able to understand new sentences we have never encountered before, expressing thoughts we have never previously entertained. Our understanding of such a sentence cannot therefore constitute our understanding of its constituents: we must already have been able to isolate the contributions made by them to the thoughts expressed by other sentences containing them, so as to put them together to compose that expressed by the new sentence. But what is it to come to grasp in advance this sense attributable to a specific expression capable of occurring in a wide variety of sentences? Is it to learn the sense of that expression taken on its own? That would violate the context principle (considered as applying to sense). It is meaningless to speak of grasping the sense of an expression conceived as standing on its own, independently of any sentence in which it occurs. Its sense just *is* its contribution to thoughts expressed by sentences of which it is part; to regard the expression as standing on its own, independently of any sentence, is to destroy the whole conception of its possessing a sense.

The escape from this dilemma requires us to regard sentences, and the thoughts they express, as ordered by a relation of dependence: to grasp the thoughts expressed by certain sentences, it is necessary first to be able to grasp those expressed by other, simpler, ones. To grasp the sense of a given expression requires us to be able to grasp the thoughts expressed by certain sentences containing it: if it did not, we should be able to grasp that sense in isolation, contrary to the context principle. Not, however, of *all* sentences

containing it, but only of certain ones: those of a particular simple form, characteristic for the expression in question. The contribution of the expression to the thoughts expressed by other, more complex, sentences is then grasped, and can be explained, by reference to the senses of those simpler characteristic sentences. An obvious case is that of a predicate. Someone may be credited with a grasp of the sense of the predicate if he knows the condition for it to apply to any one given object, which is to say that he has a general understanding of atomic sentences in which it figures. He need not understand *all* of them, of course, since there may be many proper names of objects that he does not know; but he must understand some singular terms picking out objects of which the predicate may intelligibly be said to hold good, and thus have the general conception of referring to such an object in the course of applying the predicate to it. The speaker's grasp of the sense of the predicate does not, however, require him to understand quantified sentences containing it. His coming to understand them is wholly a matter of his coming to grasp the senses of the quantifiers. That will depend upon his already having the conception of the predicate's being true or false of any one given object; and that in turn is derived from his prior general understanding of atomic sentences containing it.

The classic case is that of the logical constants (including the quantifiers). A logical constant can stand within the scope of another logical constant. Frege perceived, however, that, in order to give the sense of a logical constant in all contexts, it is sufficient to describe its contribution to determining the truth-conditions only of sentences of which it is the principal operator. For the truth-conditions of complex sentences depend systematically on those of their immediate constituents, the subsentences to which their principal operators are applied; here we must treat an immediate constituent of a quantified sentence as an application of the (in general complex) predicate to any one specific object. Hence, in analysing the sense of a complex sentence in which some logical constant figures as a non-principal operator, we have successively to consider ever simpler constituents; by the time we attend to the contribution made by the given logical constant, we shall be considering a constituent of which it is the principal operator.

The context principle applies to all expressions, and in particular to all singular terms, those for actual as well as those for non-actual objects. It therefore rules out that conception of a grasp of the meaning of a proper name as consisting in a direct mental apprehension, unmediated by language, of the object named and an association of the name with it. On the contrary, an understanding of the name, as of all other expressions, comprises a grasp of what determines the truth-value of a member of some characteristic range of sentences containing it. There is no such thing as an immediate apprehension of an object: it is only by coming to grasp the use of proper names, or other terms, referring to them that we form any conception of objects as persisting

constituents of a heterogeneous, changing reality and as identifiable as the same again. Mere presentation of the object fails to determine how it is to be recognised as the same again, unless some criterion of identity with which we are already familiar is presumed; and we can become familiar with such a criterion only by coming to master the use of terms for objects of that sort.

When the term stands for some actual object, we may take the most basic characteristic sentences containing it, a mastery of which is required for an understanding of the term, to be what were called in *Frege: Philosophy of Language* 'recognition statements': that is, statements of the form 'This is *T*', where *T* is the term in question, or 'This *S* is *T*', where *S* is a sortal such as 'person', 'street', 'river', etc., that is, a general term carrying with it a criterion of identity. The presence of a demonstrative pronoun or adjective is essential to a recognition statement; such recognition statements can be regarded as basic, when the object is an actual one, because actual objects are characterised as ones that can affect our senses, and hence as ones that we can perceive. Doubtless, someone whose use of a proper name was confined to recognition statements might be denied as yet to have a complete grasp of the sense of the name; to attain that, he must surely learn the use of atomic sentences applying some predicate to the object named. But the understanding of such predications may plausibly be thought in turn to rest upon a grasp of predications in which the object is identified demonstratively; if so, recognition statements are more basic than any other sentences containing the name. It is also true that we use a great many proper names of objects that no longer exist, or are too large, too small or too remote for us to be able to perceive. But it is at least arguable that our conception of what it is to pick out an object by the use of such a name is founded upon our grasp of the use of names of objects of the same or related sorts which we can encounter or perceive.

Abstract terms

Non-actual objects cannot be perceived, and they cannot be indicated, save by deferred ostension, by means of demonstratives. Recognition statements, properly so called, cannot therefore exist in their case. The definability of one expression in terms of others is only an extreme instance of the relation of dependence of sense; more usually, it is merely that the understanding of certain sentences presupposes an understanding of others, without there being any possibility of replacing the former by the latter. In neither of the cases in which we are primarily interested – that of terms for numbers and that of terms for value-ranges – does the explanation Frege offers take the form of a contextual definition. As we saw, this would still have been true even if he had not given an explicit definition of the cardinality operator, but had rested content with the method of introducing it, by means of the original equivalence alone, canvassed in §§ 63–5 of *Grundlagen* and misleadingly called by him a

'contextual definition'; it was not a contextual definition, nor a definition of any kind, since it did not permit elimination of the cardinality operator from every sentence. Wright was therefore quite wrong to treat contextually defined terms as the central case for evaluating the claim of the context principle to justify an ascription of reference to terms for abstract objects.

We thus need to approach the question afresh for the crucial cases of numbers and value-ranges. Wright undoubtedly has strong grounds for attributing to Frege a robust view for these cases. Frege quite explicitly claimed, in *Grundgesetze*, to have secured determinate references for value-range terms; obviously it was his intention in *Grundlagen* to do so for numerical terms. If there were any suspicion that he meant an attribution of reference to them to be understood as a mere *façon de parler*, it would be dispelled by his explanation in *Grundgesetze* of the (first-order) quantifier, which proceeds along what have become standard objectual lines. The domain over which the individual variables of the formal system of *Grundgesetze* are to be taken as ranging consists, it appears, solely of the two truth-values together with the value-ranges, i.e. the referents of sentences and of terms formed by means of the abstraction operator. Yet his stipulation regarding the universal quantifier, in § 8 of *Grundgesetze*, Volume I, reads thus:

' $\text{—}\overset{a}{\text{—}}\text{—}\Phi(a)$ ' is to refer to the value *true* if the value of the function $\Phi(\xi)$ is the value *true* for every argument, and to refer to the value *false* otherwise.

Frege does not here give a substitutional explanation of the quantifier. He does not say that ' $\text{—}\overset{a}{\text{—}}\text{—}\Phi(a)$ ' is to have the value *true* just in case, for every value-range term ' t ' that can be constructed in the formal language, ' $\Phi(t)$ ' has the value *true*. He conceives of every functional expression ' $\Phi(\xi)$ ' that can be constructed in the language as having as its referent a function which is defined on every object in the domain; ' $\text{—}\overset{a}{\text{—}}\text{—}\Phi(a)$ ' will have the value *true* just in case this function has the constant value *true*, and the value *false* in every other case. There is therefore no doubt that Frege is assuming that, by his stipulations concerning the abstraction operator, he has not only determined a genuine reference for every value-range term, but has also determined just which objects compose the domain.

The discussion in the foregoing paragraph appears to be vitiated by its neglect of the radical difference, stressed in the last chapter, between the framework of *Grundlagen* and that of *Grundgesetze*. The difference is too wide to allow the context principle, taken as warranting the ascription of reference, to have the same content when understood within the one framework and within the other, even on the assumption, yet to be scrutinised, that Frege continued to maintain the principle in *Grundgesetze*. It is certainly true that, in *Grundgesetze*, Frege took a robust view of value-range terms, in the sense of

claiming to have secured for them a reference in the substantial sense employed within the semantic theory of that work. In the framework of *Grundlagen*, on the other hand, no such wholly robust view of numerical terms was available, because that substantial notion of reference was missing. Wright may nevertheless claim that, even at that stage, Frege's view of them had a certain degree of robustness, in that the ascription of reference to them was no mere *façon de parler*; on the contrary, Wright's use of the expression 'objectual reference' may be defended on the ground that numerical terms are understood as denoting elements of the domain of the individual variables, and quantification over that domain is understood objectually. Obviously, the latter claim can hardly be sustained by quoting the explanation of the quantifier given in *Grundgesetze*; the claim is nevertheless undoubtedly correct. If it were otherwise, it would be unintelligible that Frege allows the cardinality operator, in *Grundlagen*, to be attached to predicates applying to cardinal numbers, an operation upon which his proof of the infinity of the natural numbers depends; *Grundlagen* makes no sense unless we take the referents of numerical terms to be full-fledged objects falling within the domain of quantification. The much more loosely expressed stipulation in *Begriffsschrift*, § 11, concerning the quantifier reads:

$\vdash \text{—} \Phi(a)$ signifies (*bedeutet*) the judgement that the function is a fact whatever we take as its argument.

Fairly clearly, this, too, is intended to express an objectual interpretation of the first-order quantifier, an interpretation that Frege appears to have put on it throughout his career.

This semi-robust feature of the context principle, as understood even in *Grundlagen*, nevertheless lays an extra burden on the explanations by means of which a new range of terms is introduced, when these do not take the form of an explicit definition. It is not necessary only to determine the truth-conditions of sentences containing those terms; it is necessary also to determine those of all sentences involving quantification, that is, to determine the domain of the variables so as to include referents of all such terms.

We must bear firmly in mind that, in *Grundlagen*, Frege drew no distinction between an expression's being meaningful and there being something it stands for. The answer to the question what it is required to have for it to denote something is therefore the same as that to the question what must be known if we are to understand it: the questions are not differentiated. The answer, in both cases, is: determinate truth-conditions for sentences containing it. That is why the context principle – as employed in *Grundlagen* – makes a term's possession of a meaning internal to the language: we need only satisfy ourselves that truth-conditions have been fixed for all the sentences in which the term may occur, and no further question remains to be settled. In particu-

lar, no question can be raised whether it really stands for anything: the object for which it stands is given to us through our understanding of the term, which is in turn constituted by our grasp of the senses of sentences containing it.

We may say in reply that, even on the most resolutely internalist view, there is a further question to be settled, especially when a term-forming operator, and therewith a whole range of new terms, are being introduced: the question of suitably determining the domain of quantification. This, however, was something that Frege persistently neglected, a neglect which, as we shall see, proved in the end to be fatal.

Given Frege's insistence on there being non-actual objects, as objective as actual ones, referred to by means of abstract terms and belonging to the same domain of quantification as actual ones, Wright's case for a robust interpretation of the context principle as Frege intended it to be understood in *Grundlagen* may well seem difficult to gainsay. It cannot be gainsaid if 'robust' is taken to mean merely that there are genuine objects corresponding to the terms under consideration, and that these objects fall within the domain of the individual variables. If, on the other hand, it is taken to mean that the identification of an object as the referent of such a term is an ingredient in the process whereby the truth-value of a sentence containing it is determined, Wright's interpretation must be rejected as importing ideas from Frege's middle period foreign to *Grundlagen*. On the *Grundlagen* view, we can ask whether the truth-conditions of sentences containing a term of the kind in question have been fixed, and for a statement of those truth-conditions; we cannot ask after the mechanism by which the truth-values of those sentences are determined, nor, therefore, after the role of the given term in that mechanism.

When no definition is involved, there can be no question of an austere view in Wright's sense. When we shift to the perspective of *Grundgesetze*, there may be some view to be taken that falls short of full robustness: one that allows that a reference has in some sense been secured for the terms being introduced into the language, but denies that such reference can be construed realistically. Such a less robust view could not be called an 'intermediate' view, because there is no austere view to stand on the other side of it to the robust view: there is only the nominalist view according to which the putative abstract terms ought to be expunged from the language altogether, or at least not only denied a reference but declared incapable of occurring in true sentences.

To reject nominalism is to declare that abstract terms, as such, are unobjectionable. To recognise that there is no objection in principle to them requires acknowledgement that some form of the context principle is correct, since abstract objects can neither be encountered nor presented. The context principle in fact also governs terms for actual objects, since a grasp of a proper name involves an understanding of its use in sentences, and thus of a relatively complex segment of language. The nominalist is unaware of this, however. If

he is old-fashioned, he harbours a mythical conception of a mental connection between name and bearer. If he is more up to date, he entertains the equally superstitious belief that, for me to refer to an object, that object must have acted to initiate a casual process that eventuated in my utterance of the name. In either case, his conception of reference prompts him to regard names of actual (concrete) objects as thoroughly legitimate, and names of abstract ones as spurious. To recognise abstract terms as perfectly proper items of a vocabulary therefore depends upon allowing that all that is necessary for the lawful introduction of a range of expressions into the language is a coherent account of how they are to function in sentences, even when those expressions have the form of singular terms; and this is a version of the context principle. It is as yet unclear, however, whether acknowledging the context principle to this extent – the extent necessary for a repudiation of nominalism – carries with it a commitment to a robust or realist conception of reference as ascribed to abstract terms, or is compatible with a less robust, non-realist conception of it; nor whether, in the latter case, the satisfaction of some further condition will warrant a realist conception.

This question cannot be answered without a clearer idea of how abstract terms can legitimately be introduced into the language, when not by contextual definition. We know in advance, however, that Frege's method of introducing them was *not* legitimate: it could, and in the case of value-range terms did, lead to inconsistency. In relation to it, therefore, it is pointless to debate whether a more or less robust interpretation should be put on the notion of reference as applied to terms so introduced. This is in effect what Wright does, on the plea that, when the method is used solely to introduce terms for natural numbers, no contradiction will ensue. But, if the method *can* lead to contradiction, it is patently unsound. Our task is therefore to locate the error in Frege's procedure, and enquire whether it could be repaired: only then shall we be in a position to discuss abstract terms, including mathematical ones, in general.

CHAPTER 17

The Context Principle in Grundgesetze

Our question is: how did the serpent of inconsistency enter Frege's paradise?

Terms for logical objects must obey the same principles as all other expressions. In accordance with the context principle for sense, the senses of such terms consist in the contribution they make to the senses of sentences in which they occur. The context principle further requires that those senses cannot be thought of as given antecedently to the senses of all such sentences: they are given by the manner in which the truth-values of certain basic characteristic sentences containing them are determined. The truth-conditions of more complex sentences containing those terms are then to be regarded as understood by appeal to (or explicable in terms of) those of the more basic sentences. The contribution of one of the terms in question to the sense of any such more complex sentence can then be conceived as its contribution to the senses of one or more (possibly infinitely many) basic sentences on which the sense of the complex one depends. This holds good, as we have seen, for primitive predicates, for logical constants and for names of actual objects. The questions to be answered in any specific case are: (1) which are the basic sentences? (2) what are the truth-conditions of these basic sentences, and how do we grasp them? (3) how do the senses of the complex sentences depend on those of the basic ones? In the case of names of actual objects, the most basic sentences are, or can plausibly be taken to be, recognition statements. For terms standing for logical objects, there are no such sentences. Which, then, on Frege's account of the matter, are the basic sentences characteristic for the senses of such logical singular terms?

In *Grundlagen*, the answer is plain enough: they consist of all the identity-statements in which a term of the kind being introduced figures – both those in which a term of that kind appears on either side of the identity-sign, and those in which such a term appears only on one side. The original equivalence serves to give the truth-conditions of identity-statements of the first type; but it is rejected as a claimant for providing on its own a complete explanation of the new terms for cardinal numbers because it fails to determine the truth-conditions of identity-statements of the second type (that is, to solve the Julius

Caesar problem). We are hindered from examining the matter any further by Frege's choosing to remedy the defect by resorting to an explicit definition. Explicit definitions are always in order, provided that the expressions used in the definiens, and taken as already understood, are themselves in order. This forces us to attend to the completion, in *Grundgesetze*, of the unfinished business of *Grundlagen*.

We have accordingly to abandon our methodological assumption that, in *Grundgesetze*, Frege maintained the original context principle, but now understood as relating to reference, and examine how he actually tackled the unfinished business. He did so in Volume I, §§ 9, 10 and 31, with relevant remarks in §§ 29 and 30. The context principle, as formulated in *Grundlagen*, allots a primacy, with respect to meaning, to *sentences*: it is only in the context of a *sentence* that a word has meaning. We saw that, in § 32 of *Grundgesetze*, Volume I, Frege continued to allot a primacy to sentences, with respect to sense: the sense of an expression is its contribution to the thought expressed by a sentence in which it occurs. He refers to sentences, in this paragraph, as 'names of truth-values'. This is because, in the theory of reference of *Grundgesetze*, no categorial distinction, theoretical or formal, is drawn between truth-values and objects, or between sentences and singular terms: truth-values are just two of the objects in the domain (even if particularly distinguished ones), and a sentence is simply a singular term that happens to have a truth-value as its referent. There is thus no primacy allotted to sentences, with respect to reference. In so far as the primacy of sentences is an integral part of the content of the context principle, as it figures in *Grundlagen*, Frege did *not* maintain that principle, as relating to reference, in *Grundgesetze*.

He did maintain a generalised context principle, however, which is fundamental to his attempted justification for introducing the abstraction operator. In Volume I, §§ 3 and 9, he states the principle (embodied, in § 20, in his celebrated Axiom V) that the value-range $\hat{\epsilon}f(\epsilon)$ of a function $f(\xi)$ coincides with the value-range $\hat{\alpha}g(\alpha)$ of a function $g(\xi)$ just in case, for every $\mathbf{a}$, $f(\mathbf{a}) = g(\mathbf{a})$. This is the criterion by which we can recognise a value-range as the same again, when it is designated by a value-range term, that is, one of the form ' $\hat{\epsilon}\Phi(\epsilon)$ '. In § 10, Frege raises the question whether the principle is sufficient to determine the reference of each value-range term. His answer is that it is not, on the ground that, by appeal to it,

we can as yet neither decide whether an object is a value-range, if it is not given to us as such, or, if a value-range, of what function, nor in general decide whether a given value-range has a given property, if we do not know that this property is connected with a property of the function to which it belongs.

This is the Julius Caesar problem again. From the criterion of identity between numbers, we cannot determine whether an object not given as a number, such

as England or Julius Caesar, is a number at all, and, if so, to what concept it belongs. We can determine that the number of planets is odd, and is a perfect square, since those propositions can be expressed as propositions about the concept *planet*; but we cannot determine whether or not it has a monarchy or was assassinated in the Capitol.

Frege proceeds to back this up with an argument to the effect that, given any assignment of referents to value-range terms, a permutation of them would not disturb the criterion of identity (i.e. that Axiom V would remain true). This argument appears to flout the context principle: for a similar argument would defeat any claim to have fixed the reference of the primitive vocabulary of any formal language (provided, in the general case, that the extensions of the primitive predicates were also subjected to the permutation).¹ When Frege gives his solution to the problem, however, it fails to meet the objection from the permutation argument; but it plainly rests upon a generalised context principle. 'How is this indeterminacy' of reference 'to be overcome?', he asks, and answers:

By determining, for each function as it is introduced, what values it obtains for value-ranges as arguments, just as for all other arguments.

He proceeds to do this for each of the functions that have so far been introduced, namely the relation of identity, the horizontal function and the negation function. The references of these have been specified as follows. In § 5, the horizontal function $\text{---}\xi$ was laid down as being that whose value is the value *true* for the value *true* as argument, and the value *false* for any other object as argument. In § 6, the negation function $\text{---}\xi$ was stipulated to have the value *false* for the value *true* as argument, and the value *true* for any other object as argument. Finally, identity of course figures in Frege's system as a binary function $\xi = \zeta$ whose value is always a truth-value. In § 7 the value of this function, for which we may here use the name 'the equality function', was stated as being the value *true* when the same object is taken both as the first argument and as the second, and the value *false* in every other case.

What is the assumption implicitly underlying Frege's alleged solution of the problem of determining the references of value-range terms? It is evidently

¹ A similar permutation argument has been used by Hilary Putnam against metaphysical realism, and by Donald Davidson to show that the notion of reference – unlike that of truth – is purely internal to a theory of meaning. The context principle belongs to the internalist strain in Frege's thinking, and rules out such a permutation argument, when taken at face value to show that reference has not been adequately determined, as spurious. Frege's appeal to it suggests that he no longer adhered to the context principle for reference in any form; but the fact that his solution to the problem of fixing the reference of value-range terms fails to meet the permutation argument on its own ground counters that suggestion by indicating that his appeal to the argument was an aberration. That aberration may be taken as indicating that his understanding of the notion of reference was indeed robust, but also the difficulty of combining a robust interpretation of it with adherence to the context principle.

that a singular term of the formal language has reference if the result of inserting it into the argument-place of any functional expression of the language has a reference. Let us label this assumption 'GCP' (for 'generalised context principle'). It is a generalisation of the context principle of *Grundlagen* in that neither sentences nor predicates play any distinguished role. It says, roughly, that the term in question has a reference provided that every more complex term of which it is a constituent has a reference. In the formal language of *Grundgesetze*, sentences are treated syntactically as singular terms; and GCP accords them no role in the theory of reference distinct from that of other complex singular terms. In that theory of reference, names of truth-values have no semantic role distinguishing them from names of other objects, despite the fact that they do have a special place in the *Grundgesetze* theory of sense.

Not only has the context principle of *Grundlagen* been generalised: it has also acquired a stronger sense. For the notion of reference with which Frege is here operating is no longer the thin notion of reference (meaning or content) of *Grundlagen*, under which metalinguistic statements about reference were to be understood by reducing them to statements of the object-language: it is the substantial notion of reference which serves as the central notion of his semantic theory. Moreover, the principle has become more specific. A mere generalisation of the principle, as stated in *Grundlagen*, would say that a term will have a reference if we have supplied a reference for every more complex term containing it; but Frege now claims that a reference will have been secured to it provided only that we have supplied a reference for every term formed by inserting it into the argument-place of every primitive unary functional expression, and of every unary first-level functional expression formed by filling one argument-place of a primitive binary first-level functional expression. Remarkably, for two such formally distinct versions of the principle, the applications Frege makes of them reduce to much the same: the solution of the Julius Caesar problem.

It so happens that all three primitive function-symbols listed by Frege as having already been introduced serve to form sentences; the functions to which they refer have only truth-values as values. In Frege's argument, however, this fact is not specifically alluded to. It is undoubtedly important to that argument that the values of those functions have been explained, unproblematically, as objects with which we are presumed to be already familiar, and hence independently of the notion of a value-range; but the fact that these objects are truth-values is not treated as being of any especial importance.

Besides GCP, a further assumption underlies the argument of § 10. This is that, if the result of inserting a term into the argument-place of every *primitive* functional expression has a reference, then the result of inserting it into the argument-place of *any* functional expression will have a reference. We may call this the 'compositional assumption'.

Frege then proceeds to argue as follows. First, the negation function can be left out of account, since its argument can always be taken as being a truth-value. That is to say, the value of the function $\neg\xi$ will always be the same as that of $\neg(\neg\xi)$; we therefore need not consider the result of inserting a value-range term directly into the argument-place of the expression ' $\neg\xi$ '. The case of the horizontal function may be reduced to that of the equality function: for the value of the function $\xi = \eta$ is always the same as that of the function $\xi = (\xi = \eta)$. We have therefore to consider only the equality function. Thus, in the end, despite the greater generality of the underlying principle, the problem comes down once more to determining the truth-conditions of identity-statements, that is, to solving the Julius Caesar problem for value-ranges.

We have to stipulate the value of the equality function for any case in which one of its arguments is given as a value-range and the other is not. Frege here observes that 'we have so far introduced as objects only the truth-values and the value-ranges'; that is to say, all terms other than value-range terms, formed by means of the abstraction operator, have been stipulated to have truth-values as their referents. Hence, Frege argues, the matter reduces to the question 'whether either one of the truth-values is a value-range'.

The Julius Caesar problem solved

Frege now gives an argument to show that we are at liberty to make a transsortal identification of the value *true* with any arbitrary value-range, and of the value *false* with any other arbitrary value-range distinct from it. The argument is essentially as follows. Suppose that we have a domain of value-ranges, with none of which the two truth-values coincide. (More rigorously stated, suppose that we have a model of the system, in which no value-range term has the same denotation as any sentence.) Choose any two extensionally non-equivalent functions h and j expressible in the system. Define a function X which maps the value *true* on to the value-range of h , and conversely, the value *false* on to the value-range of j , and conversely, and every other object on to itself. We may then reinterpret terms of the system as follows: we continue to treat sentences as denoting one or other truth-value, just as before; but we take a value-range term to denote the result of applying the function X to that element of the domain which it denoted in the original model. The resulting interpretation will still satisfy Axiom V, and will yield a model in which the two truth-values are also value-ranges.

Since we are at liberty to identify the values *true* and *false* with any two value-ranges we choose, Frege elects to identify the value *true* with its unit class (the value-range of the horizontal function) and the value *false* with its unit class (the value-range of the function which maps the value *false* on to the value *true*, and every other object on to the value *false*.) Therewith, the

Julius Caesar problem for value-ranges is solved: the truth or falsity of a statement identifying a value-range with a truth-value will now be determined by the criterion of identity for value-ranges, since each of the truth-values now is a value-range. The task of determining the references of value-range terms is thereby completed, for the time being; for, as Frege remarks, 'As soon as there is a further question of introducing a function that is not completely reducible to the functions already known, we can stipulate what values it is to have for value-ranges as arguments; and this can be regarded as being as much a determination of the value-ranges as of that function'.

Conditions for referentiality

In §§ 29–31, Frege returns to the matter in more detail, essaying, in § 31, a proof that every singular term of his symbolism has a determinate reference; this would include as a corollary that every sentence had a determinate truth-value. From this fuller treatment, we can discern his intentions more precisely. In § 29, he lays down general conditions for an expression of each logical type to be said to have a reference. Frege of course regarded a binary function as a function of two arguments, not as a function of one argument whose value was again a function of one argument; as already noted, he admits only functions whose values are objects (including truth-values). But, for the purposes of these sections, he allows for the formation of an expression for a function of one argument by inserting a singular term in *one* of the argument-places of an expression for a function of two arguments. Then, if for ease of formulation we say that an expression is 'referential' if it has a reference, his main stipulations are as follows:

(i) an expression for a first-level function of one argument has a reference provided that the result of inserting a referential term in its argument-place is always again a referential term;

(ii) a singular term ('proper name') has a reference if

(a) the result of inserting it in the argument-place of a referential expression for a first-level function of one argument is always a referential term; and

(b) the result of inserting the given term in either of the argument-places of a referential expression for a first-level function of two arguments is a referential expression for a first-level function of one argument;

(iii) an expression for a first-level function of two arguments has a reference if the result of filling both of its argument-places with referential singular terms always has a reference;

(iv) an expression for a second-level function which takes a first-level function of one argument as its sole argument has a reference if the result of inserting in its argument-place a referential expression for a first-level function of one argument always has a reference.

A high degree of circularity is evident in these stipulations: to determine whether an expression for a unary first-level function has a reference, we have to know whether, when we insert in its argument-place a singular term that has a reference, it yields a more complex term that again has a reference; and to determine whether a singular term has a reference, we have to know whether, when we insert it in the argument-place of a functional expression that has a reference, it yields an expression that has a reference. Frege is in effect satisfied, however, that, provided that there are some expressions of which we can assert outright that they have a reference, we can use his stipulations as inductive clauses from which to derive that other, in general more complex, expressions have a reference.² The stipulation governing the reference of singular terms clearly embodies the GCP.

Frege's consistency proof

Armed with the stipulations of § 29, Frege proceeds in § 31 to set out his purported proof that every term of his symbolism has a reference. He deals with the interdependence of function-symbols and singular terms by establishing the referentiality of the primitive function-symbols with respect to a fragment of the language containing terms for truth-values only. Relatively to this fragment, expressions for the horizontal, negation, equality and conditional functions are all referential, the functions for which they stand having determinate values for truth-values as arguments. The treatment of the universal quantifier is worth quoting. We have to ask, Frege says,

whether it follows generally from the fact that the function-name ' $\Phi(\xi)$ ' refers to something that ' $\underbrace{\quad}_{\alpha} \Phi(\alpha)$ ' has a reference. Now ' $\Phi(\xi)$ ' has a reference if, for every referential proper name ' Δ ', ' $\Phi(\Delta)$ ' refers to something. If so, this reference is either always the value *true* (whatever ' Δ ' refers to), or not always. In the first case ' $\underbrace{\quad}_{\alpha} \Phi(\alpha)$ ' refers to the value *true*, in the second to the value *false*. It thus follows generally from the fact that the inserted function-name ' $\Phi(\xi)$ ' refers to something, that ' $\underbrace{\quad}_{\alpha} \Phi(\alpha)$ ' refers to something. Consequently the function-name ' $\underbrace{\quad}_{\alpha} \Phi(\alpha)$ ' is to be included in the circle of referential names. This follows in a similar way for ' $\underbrace{\quad}_{\beta} \mu_{\beta} (\mathfrak{F}(\beta))$ '.

² The first sentence of § 30 runs: 'These propositions are not to be construed as definitions of the words "to have a reference" or "to refer to something", because their application always assumes that some names have already been recognised as having a reference; they can however serve to widen, step by step, the circle of names so recognised.' The word 'names' here covers meaningful expressions of all logical types.

So far, all is unquestionably correct – if not very interesting – when the domain is taken to consist of just two objects, the two truth-values *true* and *false*. But now Frege has to deal with the abstraction operator. He realises that the problem is of a different kind:

The matter is less simple for ' $\hat{\epsilon}\phi(\epsilon)$ '; for, with it, we do not merely introduce a new function-name, but at the same time a new proper name (value-range name) for every name of a first-level function of one argument.

This is misleadingly stated. With the description operator we introduce new singular terms ('proper names'); but, by the meaning of the description operator, they will all refer to objects already in the domain. With the introduction of the abstraction operator, on the other hand, new *objects* are introduced: that is, the domain itself must be greatly extended. Frege appeals to his criterion for the referentiality of an expression for a second-level function. The function-name ' $\hat{\epsilon}\phi(\epsilon)$ ' will have a reference if, for every referential expression ' $\Phi(\xi)$ ' for a first-level function of one argument, ' $\hat{\epsilon}\Phi(\epsilon)$ ' has a reference. Frege therefore restricts attention to value-range terms ' $\hat{\epsilon}\Phi(\epsilon)$ ' formed from a referential function-name ' $\Phi(\xi)$ ', calling these 'proper' value-range terms.³ To decide whether a given proper value-range term ' $\hat{\epsilon}\Phi(\epsilon)$ ' has a reference, he appeals to his criterion for the referentiality of a singular term: for every referential expression ' $\Psi(\xi)$ ' for a function of one argument, ' $\Psi(\hat{\epsilon}\Phi(\epsilon))$ ' must have a reference, and, for every referential expression ' $\Theta(\xi, \zeta)$ ' for a function of two arguments, ' $\Theta(\xi, \hat{\epsilon}\Phi(\epsilon))$ ' and ' $\Theta(\hat{\epsilon}\Phi(\epsilon), \xi)$ ' must have a reference. At this point, Frege tacitly appeals to his compositional assumption that, if the condition holds for all primitive function-names, it will hold for all function-names whatever. He therefore repeats the argument of § 10, appealing to the identifications there made of the two truth-values with their unit classes. The only additional function-symbol he has to consider is the conditional, and this is dealt with in the same way as the symbol for the negation function. The only remaining primitive symbol is the description operator, and this is easily treated.

Frege concludes that he has demonstrated that every singular term of his symbolism has a determinate reference. He most certainly had not. If he had, he would have given a consistency proof; his first reaction, on learning from Bertrand Russell of the contradiction, was to write to him that

my reasonings in § 31 do not suffice to ensure a reference in all cases for my complex symbols.⁴

What had gone wrong?

³ 'Fair course-of-values-names' in Montgomery Furth's translation *The Basic Laws of Arithmetic: Exposition of the System* (University of California Press, 1964) of part I of *Grundgesetze*.

⁴ Letter of 22 June 1902.

How the serpent entered Eden

James Bartlett, in his unjustly neglected doctoral thesis, a highly perceptive study of Frege's ontology and semantics,⁵ puts the blame on the compositional assumption: he allows that Frege proves that each of his primitive functions has a determinate value for the referent of any value-range term as argument, but denies that he proves this for every function expressible in the notation. It is difficult at first sight to see how this can be. Functional expressions are, in general, built up by iteration of the primitive function-names: so an inductive argument ought to suffice to justify the compositional assumption. It is true that, as Frege viewed the matter, another operation is involved, namely the formation of an expression for a first-level function of one argument by removing, from a singular term, one or more occurrences of a constituent singular term. This operation, on Frege's conception, is a necessary preliminary to the formation of a value-range term or of a quantified sentence by application of the abstraction operator or the first-order universal quantifier. Concerning it, he simply remarks without proof in § 30 that:

The function-name obtained in this way . . . always has a reference if the simple names out of which it is formed refer to something.

That this assertion is correct can most easily be seen if we appeal from Frege's conception of the formation of complex terms and sentences to Tarski's. On Tarski's conception, the process of formation takes as its starting point, not closed terms, but open terms and open sentences containing free variables. We need then to apply, not the notions of absolute denotation and absolute truth-value, but those of denotation and truth-value relative to some assignment of elements of the domain to the free variables; but the operation of forming a complex functional expression or complex predicate by extracting a term from a closed complex term or closed sentence is rendered otiose.

Despite appearances, Bartlett is nevertheless right. The second-order quantifier presents an altogether different problem; and it is to its presence in Frege's formal language that the contradiction is due. It was indispensable for Frege's purposes, since it was only by means of it that he could define his application operator $\cap$, $a \cap g$ being the value for the argument a of the function whose value-range is g ; when g is a class, $a \cap g$ is the truth-value of ' a is a member of g '. Frege's explanation of the second-order quantifier, unlike that which he provides for the first-order one, appears to be substitutional rather than objectual. In § 25 he says

Let $\Omega_\beta (\phi(\beta))$ be a second-level function of one argument of the second kind [namely a first-level function of one argument], whose argument-place is indicated

⁵ James M. Bartlett, *Funktion und Gegenstand*, Munich, 1961.

by ' ϕ '. Then $\text{---}_{\beta}^{\mathfrak{F}} \Omega_{\beta}(\mathfrak{F}(\beta))$ is the value *true* only when for every suitable argument the value of our second-level function is the value *true*.

Obviously, this is a comment, not a stipulation, since it is not laid down what $\text{---}_{\beta}^{\mathfrak{F}} \Omega_{\beta}(\mathfrak{F}(\beta))$ is to be when the condition is not fulfilled; and no explanation is given of what constitutes a 'suitable argument'. The comment must be taken as appealing to the pronouncement in § 20:

Now we understand by ' $\text{---}^{\mathfrak{F}} \text{---} \mathfrak{F}(\Gamma)$ ' the truth-value of one's always obtaining a name of the value *true* whichever function-name one inserts in place of ' $\mathfrak{F}$ ' in ' Γ ').

Despite the lack of generality, this is the closest Frege comes in *Grundgesetze* to stipulating the reference of a term formed by means of the second-order quantifier. His amazing insouciance concerning the second-order quantifier was the primary reason for his falling into inconsistency. The argument of § 31 requires a proof of the legitimacy of the general operation for forming second-level function-names. Suppose given a sentence or other singular term. As a preliminary for attaching an initial occurrence of the second-order quantifier, we must form from it a second-level function-name by omitting one or more occurrences of some first-level function-name. To say that the resulting second-level function-name had a reference would be to say that every result of filling its argument-place with a first-level function-name had a reference; it would then follow that the sentence resulting from attaching the second-order quantifier had a reference. But how could the premiss be established? It might occur to us to reason by induction that, if every term containing n occurrences of the second-order quantifier has a reference, and if it is determinate, for every such term, whether or not its referent is the value *true*, then a term with $n + 1$ occurrences of the second-order quantifier must have a truth-value as its referent. But such reasoning, intended to fill a gaping void in Frege's proof, would be fallacious.

The fallacy lies in the fact that, in considering the results of filling the argument-place of the second-level function-name, we have to consider *all* first-level function-names as candidates for filling that argument-place, and these will include ones with an unbounded number of occurrences of the second-order quantifier: the induction hypothesis therefore does not suffice for our purposes. Suppose, for instance, that we abbreviate the first-level function-name 'for every $\mathfrak{F}$, if ξ is the value-range of $\mathfrak{F}$, then $\mathfrak{F}(\xi)$ ' as ' $h(\xi)$ '.⁶ We may then consider the second-level function-name 'if the value-range of h is the value-range of ϕ , then ϕ (the value-range of h)'; by attaching the second-order quantifier, we obtain 'for every $\mathfrak{F}$, if the value-range of h is the

⁶ I here use English in place of Frege's symbolism for the sake of clarity: I intend the expression to be understood as written in the primitive notation of *Grundgesetze*, however.

value-range of $\mathfrak{F}$, then $\mathfrak{F}$ (the value-range of h)'. This quantified sentence will have the value *true* just in case we obtain a true sentence by filling the argument-place of our second-level function-name by any arbitrary first-level function-name. In particular, we might fill it by the function-name ' $h(\xi)$ ', in which case we should obtain the sentence 'if the value-range of h is the same as the value-range of h , then h (the value-range of h)'. Since the antecedent is readily demonstrable, this is equivalent to ' h (the value-range of h)', which, when partially spelled out, is 'for every $\mathfrak{F}$, if the value-range of h is the value-range of $\mathfrak{F}$, then $\mathfrak{F}$ (the value-range of h)'. This, however, is precisely the sentence whose truth-value we are trying to determine. Thus the stipulations intended to secure for it a determinate truth-value go round in a circle. If, instead of ' $h(\xi)$ ', we had here taken ' $g(\xi)$ ', abbreviating 'for every $\mathfrak{F}$, if ξ is the value-range of $\mathfrak{F}$, then not $\mathfrak{F}(\xi)$ ', we should, with a little help from Axiom V, have obtained the Russell contradiction.

Without second-order quantification, Frege's formal system would be paralysed, but the set-theoretic paradoxes would not be derivable. A model for the first-order fragment of the theory could be arrived at in the following way. Let D_0 consist of the two truth-values together with the natural numbers. For any n , let D_{n+1} be the union of D_n with the set of all its finite and cofinite subsets. The domain D is then to consist of every member of any of the sets D_n , for any finite n . In the resulting model, a natural number k , considered as an element of D (or a member of the transitive closure of an element of D), is to be identified with the set of all subsets of D having exactly k members. It will be found that D contains all value-ranges definable by means of the limited vocabulary. (One might have expected, instead of 'finite and cofinite subsets', to have to say 'functions, taken in extension, whose values differ from one another for only finitely many arguments'; but, since a set is to be taken as the value-range of a function having only truth-values as values, and all Frege's primitive function-symbols other than the abstraction and description operators denote such functions, it is sufficient to construct the entire model out of sets.) In view of the consistency of the fragment of the language without the second-order quantifier, it is therefore pertinent to ask whether the proof of § 31 would have been valid for that fragment.

The most natural diagnosis of the error in the proof is that Frege fails to pay due attention to the fact that the introduction of the abstraction operator brings with it, not only new singular terms, but an extension of the domain. As we saw previously, it may be seen as making an inconsistent demand on the size of the domain D , namely that, where D comprises n objects, we should have $n^n \leq n$, which holds only for $n = 1$, whereas we must have $n \geq 2$, since the two truth-values are distinct: for there must be n^n extensionally non-equivalent functions of one argument, and hence n^n distinct value-ranges. But this assumes that the function-variables range over the entire classical totality of functions from D into D , and there is meagre evidence for attributing such

a conception to Frege. His formulations make it more likely that he thought of his function-variables as ranging only over those functions that could be referred to by functional expressions of his symbolism (and thus over a denumerable totality of functions), and of the domain D of objects as comprising value-ranges only of such functions. We therefore have to locate the error in his attempted proof more precisely.

Although the assimilation of sentences to terms in the *Grundgesetze* blocks an overt statement of the context principle as allowing a distinguished role for sentences, it is surprising how close in practice Frege comes to conforming to such an ungeneralised version of the principle. The natural way to lay down the semantics of a formal language is to start by delineating the domain of the individual variables, and then give the intended interpretations of the primitive symbols in relation to that; and this has of course become the standard way. Frege does nothing of the kind. He indicates that the two truth-values are to belong to the domain; and they are the only two objects that he assigns directly to terms as their referents. Having first introduced various symbols capable of forming only terms referring to truth-values, he then introduces value-range terms. He does not stipulate directly what the referents of these are to be, and employs no vocabulary for doing so. Instead, he argues that he has provided a reference for each value-range term, by means, in effect, of having determined the truth-value of any sentence in which that term occurs. How would it have been if he had gone about the task in what has become a more orthodox manner?

The intended domain of a formal language may be specified either by external or by internal means. An external specification characterises it as comprising certain objects with which we are presumed already familiar – the natural numbers, for example. We shall then in general need some means of singling out in the metalanguage particular elements of the domain so characterised. An internal specification requires only a comprehensive means of singling out any element of the domain, without identifying those elements with objects given in any other way. An external specification is not, of course, external to *language* as such – it is internal to the metalanguage – but only to the object-language. An internal specification nevertheless appears in better accord with Frege's employment of the GCP in *Grundgesetze*: he certainly infers from that principle that an external specification is not *required*. In order to effect an internal specification of the domain, we need indices, drawn from an already known index set, by means of which to pick out individual elements: we can, when convenient, first specify the index set, without presuming that different indices determine distinct elements, and subsequently lay down the condition for two indices to determine the same element. After the domain has been specified, the primitive predicates and individual constants have then to be interpreted with respect to that domain; but this can be done only when the criterion of identity between elements with different indices has been laid

down, since otherwise a predicate might be interpreted inconsistently, as being true of an element as picked out by one index and false of the same element as picked out by another.

In our case, the domain is to consist of the two truth-values and of value-ranges. The simplest choice of an index set for the value-ranges is that of first-level functional expressions of one argument of the formal language. Two function-names will be taken to be indices of the same value-range just in case, for every singular term, the results of inserting that term in the argument-places of those function-names refer to the same element. This, of course, merely repeats Frege's own stipulations, save for taking the function-names into the metalanguage as indices. Doing so makes glaring a circularity in the procedure: we cannot determine whether two functional expressions are indices of the same value-range until we have fixed the interpretations of the primitive symbols, and we cannot safely do this until we know which indices relate to the same value-ranges. If we decline to follow Frege's lead, and specify instead that no value-range is identical with either truth-value, we can take the extensions of the horizontal, negation and conditional functions as determined. But the equality function is a different matter: its value for value-ranges as arguments, considered as given by their indices, depends on determining whether those indices denote extensionally equivalent functions, which is a matter of the truth or falsity of a universally quantified identity-statement. In general, in order to obtain a determinate interpretation of a formal language, we must first specify, without circularity, what the elements of the domain are to be, before we go on to specify the intended interpretations of the primitive predicates; and this applies even if the only primitive predicate that gives any difficulty is the sign of identity. To specify the domain, we must at least have an index set; if we do not assume that distinct indices always determine distinct elements, we must say when they do and when they do not. It is only after we have so specified of what the domain is to consist that we are at liberty to specify the relation denoted by the identity-sign in the simple manner adopted by Frege, namely as holding between an element *a* and an element *b* of the domain just in case they are the same. Frege, on the other hand, omits to specify the domain, and, having explained the sign of identity in this manner, proceeds to lay down the condition for the truth of a statement of identity between value-ranges under the guise of fixing the reference of the abstraction operator. That will depend upon the truth of a universally quantified statement, of complexity depending on the function-names out of which the value-range terms were formed. The truth-value of that statement will in turn depend upon the application of some complex predicate to every element of the domain, and hence, in effect, upon the truth-value of every result of inserting a value-range term in its argument-place. Since these statements are likely to involve further identity-statements between value-range terms of unbounded complexity, Frege's stipulations are not well founded: the truth-value of an

identity-statement cannot be construed as depending only on the references of less complex terms or on the truth-values of less complex sentences. Although there is in fact no danger of inconsistency in the fragment of Frege's system with only first-order quantification, he has provided no valid proof of its consistency, because he has not succeeded in specifying the references of all its terms. For that reason, he has failed to justify the introduction of value-ranges.

CHAPTER 18

Abstract Objects

What the contradiction signified for Frege

The inconsistency of Frege's *Grundgesetze* system was not a mere accident (though a disastrous one) due to carelessness of formulation. He discovered, by August 1906, that it could not be put right within the framework of the theory, that is, with the abstraction operator as primitive and an axiom governing the condition for the identity of value-ranges: but the underlying error lay much deeper than a misconception concerning the foundations of set theory. It was an error affecting his entire philosophy.

The context principle is crucially important to the philosophy of arithmetic presented in *Grundlagen*; and its generalised version is of equal importance to *Grundgesetze*. Frege was engaged in completing the work that Bolzano had begun, of expelling intuition from number theory and analysis (while leaving it its due place in geometry). Bolzano had thought it important to prove fundamental results in real analysis – the mean value theorem, for instance – by methods proper to the subject, and so without appeal to geometrical intuition, even though, when conceived in terms of their geometrical representation, they appeared self-evident. It seems obvious to intuition that the graph of a function which assumes a negative value for $x = 0$ and a positive value for $x = 1$ must cross the x -axis somewhere in the interval; but it *can* be proved without appeal to intuition, and therefore *must* be. This is partly because we owe it to the subject to prove any truth we are concerned to assert if it is capable of proof; but also because what appears self-evident may not be true. It seems obvious that a curve contained within a finite interval must have a tangent at all but finitely many points; by being the first to construct a continuous function nowhere differentiable within an interval, Bolzano showed this to be false. In *Grundgesetze* Frege characterised the aim of *Grundlagen* as having been 'to make it probable that arithmetic is a branch of logic and does not need to borrow any ground of proof from experience or from intuition'.¹ The full question with which *Grundlagen*, § 62, opens is, 'How, then, are numbers

¹ *Grundgesetze*, vol. I, p. 1.

to be given to us, if we can have no ideas or intuitions of them?'. If intuition was really to be expelled from arithmetic, an answer to this question was imperatively demanded. If it could not be provided, then nothing would remain but to fall back, after all, upon either a physicalistic or a psychologistic conception of number, or else to do what was contrary to all Frege's instincts,² but which in the last year of his life he felt himself driven to do, reduce arithmetic to geometry. It was the context principle that enabled him to explain how numbers could be given to us, yet neither by intuition nor by inner perception.

Frege came to call classes and value-ranges, including of course numbers of all kinds, 'logical objects'. Why 'logical'? The term here does not have reference to what is required for *inference*: Frege was not claiming, therefore, that overt reference to classes is essential for reasoning. Cardinality generally, and numbers in particular, indeed enter into deductive inference, and so numbers qualify on this score as logical objects; but classes – extensions of concepts – are to be recognised as logical objects independently of the identification of numbers as special cases of them. It would have seemed obvious to anyone at the time that the notion of the extension of a concept was a logical one: it was precisely in a treatise on logic that one would expect to encounter it. Admittedly, what was said about extensions of concepts in such a treatise could probably be expressed in terms of the extensional properties of concepts, rather than of their extensions considered as objects and hence as falling under further concepts; but that does not of itself impugn their logical status when so considered.

The term 'logical', in the phrase 'logical objects', refers to what Frege always picked out as the distinguishing mark of the logical, its generality: it does not relate to any special domain of knowledge, for, just as objects of any kind can be numbered, so objects of any kind can belong to a class. The style of objection to logicism now exceedingly frequent is therefore quite beside the point: the objection, for instance, that set theory is not part of logic, or that it is of no interest to 'reduce' a mathematical theory to another, more complex, one. Much of this derives from hindsight, i.e. from the view we have of set theory long after the discovery of the paradoxes; but in any case it mistakes Frege's aim. By Frege's criterion of universal applicability, the notion of cardinal number is *already* a logical one, and does not need the definition in terms of classes to make it so. He did not himself speak of a 'reduction'. What we call a reduction has two parts: the proofs, from absolute fundamentals, of what we should ordinarily take as basic arithmetical laws; and the definition in terms of classes. The first is to ensure that we do not need any appeal to intuition at some early point – something not guaranteed by the mere fact that number is a logical notion. The definition in terms of classes is *not* needed to show arithmetic to be a branch of logic. To this extent, Wright is correct. Had

² See *Grundlagen*, § 19.

Frege been concerned only with number theory, and not also with analysis, and had he been able to solve the Julius Caesar problem for numbers, as he thought he solved it for value-ranges, then it would not have impaired his logicist programme to take the numerical operator as primitive. But, in definition by logical abstraction (by means of equivalence classes), Frege believed that he had found a uniform method of achieving similar results in a range of analogous cases; and classes and value-ranges were of other uses as well. To make use of them therefore afforded a great economy of apparatus.

What mattered philosophically, however, was not the definition in terms of classes, but the elimination of appeals to intuition, a condition for which was the justification of a general means of introducing abstract terms, as genuinely referring to non-actual objects, by determining the truth-conditions of sentences containing them. The contradiction was a catastrophe for Frege, not particularly because it exploded the notions of class and value-range, but because it showed that justification to be unsound. It refuted the context principle, as Frege had used it.

It is for this reason that the ontology of the late essay 'Der Gedanke' is so different from that of *Grundlagen*. In *Grundlagen* objects are divided into subjective ones – ideas – and objective ones, and the latter into actual ones – material objects like the Earth – and non-actual ones like the Equator and the numbers. In *Grundgesetze* the emphasis is upon logical objects, although Frege warns that the realm of the objective may not be exhausted by physical and logical objects.

We can distinguish between physical and logical objects, admittedly without an exhaustive partition being thereby given.³

In 'Der Gedanke', however, the 'third realm' of objects that are not, or not fully, actual appears to consist exclusively of thoughts and their constituent senses. *These* could not be dispensed with; and there was no need to dispense with them, since their existence could be recognised without appeal to anything resembling the context principle. For the non-actual objects of *Grundlagen* and the logical objects of *Grundgesetze*, however, Frege no longer had any philosophical justification; and so they quietly vanished from his ontology.

The problem how to introduce abstract objects would have been avoided if Frege could have dispensed with mathematical objects altogether by construing numbers of various kinds as concepts of second or higher order, beginning by building on the rejected definitions of *Grundlagen*, § 55 (with the third of them suitably amended). This was in effect Russell and Whitehead's solution, or would have been if *Principia* had been developed within the simple theory of types, rather than the ramified theory required by the vicious circle principle. Arithmetical theorems would then have been interpreted as yet more unprob-

³ *Grundgesetze*, vol. II, § 74.

lematically logical in character, and as admitting of yet more direct application. It is unclear whether Frege ever seriously considered this possibility; but, if he did, the dependence of arithmetical statements for their truth on the existence of infinitely many non-logical objects must surely have been for him a fatal obstacle.

Discovery or invention?

Where, then, do we stand? We cannot retreat to the nominalist fatuity of regarding a belief in the existence of the Equator as a baseless and unexplanatory superstition. At the other extreme, Crispin Wright's claim that we should accord to contextually defined abstract terms a genuine, full-blown reference to objects must be dismissed as exorbitant. Yet Frege's attempted justification of the introduction of a range of abstract terms not explicable by contextual (or other) definition proved to be fallacious. It seems that there is no ground left to stand on.

If Frege had been able to devise a solution to the Julius Caesar problem for numbers that he considered as adequate as his solution of the problem for value-ranges, then, as already remarked, his programme for number theory alone would not have been essentially affected by taking the cardinality operator as primitive and as governed by the original equivalence (the criterion of identity for numbers) as an axiom. There would then have been no inconsistency; but the claim to have determined the truth-value of every sentence of the theory would have been as fallacious as the parallel claim for the language of *Grundgesetze*. The criterion of identity governs identity-statements connecting terms formed by attaching the cardinality operator to predicates defined over a domain that includes the referents of those terms (the numbers); the criterion is expressed by means of a statement involving those predicates. Suppose it had been presented as an axiom governing the cardinality operator, taken as primitive, as Axiom V governs the abstraction operator in *Grundgesetze*; and suppose that, on the strength of the context principle, Frege had invoked it as serving partially to determine the references of numerical terms, as, in *Grundgesetze*, he takes Axiom V to do for value-range terms. This would mean, in effect, that he was taking it as a partial specification of what cardinal numbers there were; the specification would be completed by a solution to the Julius Caesar problem. So considered, it would be as objectionably impredicative as the analogous specification for value-ranges: for the truth of any statement of identity between numbers would depend on the extensions of two predicates defined on a domain which included the cardinal numbers, and whose composition the axiom was supposed to be playing an essential part in determining. It is just this feature which enabled Frege to prove, from the fundamental equivalence, the infinity of the sequence of natural numbers, by showing, for any natural number n , that the number of numbers $\leq n$ is a (the)

successor of n . It is also this feature which blocked the original equivalence from being part of a genuine contextual definition of the cardinality operator. But, just for this reason, it did not succeed in determining the truth or falsity of every statement of identity between numbers. An obvious example would be the status of Aleph-0 (called by Frege in *Grundlagen* $\aleph_1$). The largest number would evidently be the number of all objects, i.e. the number belonging to the concept *identical with itself*: the question whether this number was the same as or different from the number of natural numbers would be left quite undetermined by Frege's stipulations. His belief that he had in his possession a means of determining the truth-value of every statement of a formal theory – that is, of finding a uniquely appropriate model for it – was grossly overblown.

Even had Frege felt able to renounce his claim to be able to prove the infinity of the sequence of natural numbers, and so evade the problem of justifying the introduction of mathematical objects by construing cardinal numbers as second-level concepts, and real numbers as, say, second-level relations, his second-order logic would still have been impredicative: from a statement beginning with the second-order universal quantifier he allows the inference of the statement resulting from the insertion of any first-level function-name, including one again invoking second-order quantification, in the argument-place of the second-level function-name to which the initial second-order quantifier was attached. The choice between predicative and impredicative theories involving quantification over functions, properties and relations or sets is sometimes said to depend upon whether mathematical entities are regarded as created by our thinking or as existing independently of us. We are then at a loss to know how to resolve a metaphysical issue couched in these metaphorical terms. Was the monster group *discovered* as Leverrier discovered Neptune? Or was it *invented*, as Conan Doyle invented Sherlock Holmes? How can we decide? And can the legitimacy or illegitimacy of a certain procedure of reasoning within mathematics possibly depend on our answer?

A mathematician, impressed by the ineluctability of mathematical proof, and by the unexpectedness of many mathematical results, may be impelled to insist that he discovers them. A philosopher, struck by the contrast between the intellectual labour of the mathematician and the manipulation by the astronomer of physical instruments like telescopes and spectrometers, may feel equally strongly constrained to regard the former as engaged on invention. Yet this appears to have little to do with whether the mathematician employs or abjures non-constructive methods. Though they differ about what constitutes mathematical proof, it remains as ineluctable for the intuitionist as for the classical mathematician; the results obtained by the former may surprise him as intensely as those obtained by the latter. It is pointless to debate whether the mathematician resembles the astronomer more closely than the novelist. He resembles neither in any illuminating way: no enlightenment is to be attained by choosing between two such inappropriate similes.

The contrast between mathematical and empirical enquiry concerns not so much the discovery of individual objects as the delineation of the area of search. The astronomer need have no precise conception of the totality of celestial objects: he is concerned with detecting whatever is describable in physical terms and lies, or originates, outside the Earth's atmosphere, and he need give no further specification of this 'whatever'. In mathematics, by contrast, an existential conjecture, to have any definite content, requires a prior circumscription of the domain of quantification. The difference between predicative and impredicative second-order quantification is not between a cautious and a bold assumption about what mathematical entities exist: it is between an axiomatisation that is self-explanatory and one that is not. If we are given a first-order theory of which we suppose ourselves to have a determinate interpretation, its extension by the addition of predicative quantification over properties and relations defined over the elements of the domain needs no further explanation. By assumption, we already have a clear conception of what it is for, say, a formula with one free variable, expressed in the language of the original theory (supplemented, if necessary, by terms denoting all the elements of the domain) to be true or false of any one arbitrary element. The domain of the new property-variables can then be regarded as indexed by those formulas: if they are governed by an axiom of extensionality, two such properties will be identifiable if the formulas indexing them are co-extensive. An impredicative second-order extension of the original theory, by contrast, would not be self-explanatory, since to attempt an analogous explanation would involve vicious circularity: we have already to know the range of the second-order quantifiers if we are to know what it is for a formula with one free variable, but involving second-order quantification, to be true of an element of the domain. So construed, the vicious circle principle makes no assertion about what does or does not exist: it merely distinguishes between what does and what does not require further explanation. Impredicative second-order quantification is most usually taken to gain whatever intelligibility it has from a picture we find it natural to employ. This picture invokes, first, the conception of a completed arbitrary assignment of values *true* and *false* (or numbers 1 and 0) to the elements of the given domain. 'Arbitrary' here means that the assignment does not depend upon any general rule: the values are assigned randomly (perhaps pictured as effected by successive random choices). 'Completed' means that we are conceiving of such an assignment as allotting values to all the elements of the domain. Having formed this conception, we proceed to form the further conception of the totality of *all* such arbitrary assignments: this totality forms the domain of quantification of the property-variables, understood as satisfying an impredicative principle of substitution or comprehension axiom. Whether such a picture really does yield a coherent and determinate conception of a domain of second-order quantification is a notoriously debatable question, especially when the intended domain of the first-order theory

was non-denumerable, or not even well-ordered by a known relation; this question may here be passed by. All that matters here is that such a picture – or some more sophisticated one – is needed if impredicative second-order quantification is to be understood: the impredicative comprehension axiom cannot, by itself, provide such understanding.⁴

The doctrine that, in mathematics, existence means consistency was at one time espoused by Hilbert:⁵ to ask whether mathematical entities of a given range exist is simply to ask whether any contradiction would follow from supposing them to exist. Frege utterly repudiated such a view. For him, the primary error in the widespread practice of simply ‘postulating’ the existence of some range of mathematical objects lay in the unwarranted presumption that it was consistent to do so: he insisted that the fact that no contradiction lay on the surface was no proof that none was lurking unobserved. (In *Grundgesetze*, he attempted to heed his own warnings by providing a proof of consistency; his tragedy lay in its being fallacious.) He argued, further, that the only way in which consistency *could* be proved was by demonstrating the existence of the required range of mathematical objects, that is, in our terminology, by providing a model: we cannot infer existence from consistency, but only consistency from existence. In thinking this to be the only way to prove consistency, we know him to have been wrong. (Not, indeed, wildly wrong, since most consistency proofs that do not provide a model for the whole theory proceed by determining a model for any finite subset of the axioms, and Frege had no compelling ground to consider infinite axiom-systems.) In any case, Frege considered that, even if consistency could be proved without providing

⁴ Such a more sophisticated picture is provided by progressing through the (cumulative) ramified hierarchy of properties of individuals into the transfinite, until a fixed point is reached. Here second-order properties are those expressible by means of quantification only over individuals and first-order properties; properties of order α those expressible by means of quantification only over individuals and properties of order less than α : a fixed point is an ordinal β such that all properties of order $\beta + 1$ are already of order β . The impredicative comprehension axiom will then be satisfied if the property-variables are taken as ranging over the properties of order β . This picture is assuredly not that which originally prompted the assumption that the impredicative comprehension axiom can be satisfied, but, if it is acceptable as coherent and determinate, justifies that assumption. Whether it is so or not depends upon the determinateness and coherence of the conception of the totality of ordinals necessary to prove the existence of the fixed point β .

⁵ Hilbert says this, for example, in his letter to Frege of 29 December 1899: ‘For as long as I have been thinking, writing and lecturing about these things, I have always said . . . : If the arbitrarily posited axioms, with all their consequences, do not contradict one another, then they are true, and then the things defined by the axioms exist. That is for me the criterion of truth and of existence. The proposition “Every equation has a root” is true, or the existence of roots is proved, as soon as the axiom “Every equation has a root” can be added to the remaining axioms of arithmetic without a contradiction’s being able to arise by means of any consequences drawn therefrom.’ Hilbert then refers to his lecture ‘Über den Zahlbegriff’, published in the *Jahresbericht der Deutschen Mathematiker-Vereinigung*, vol. 8, 1900, pp. 180–4, and reprinted as Appendix VI to his *Grundlagen der Geometrie*, 7th edn. In that lecture he advanced the very same thesis, as he says in his letter; in it ‘I carried out, or at least indicated, the proof that the system of all ordinary real numbers exists, and that on the other hand the system of all Cantorian powers or of all Alephs does not exist – as Cantor also asserts in a similar sense, but in slightly different words.’

a model, what mattered was the existence of a model rather than mere formal consistency.⁶ In this, he was surely correct. Whatever mathematicians profess, mathematical theories conceived in a wholly formalist spirit are rare. One such is Quine's New Foundations system of set theory, devised with no model in mind, but on the basis of a hunch that a purely formal restriction on the comprehension axiom would block all contradictions. The result is not a mathematical theory, but a formal system capable of serving as an *object* of mathematical investigation: without some conception of what we are talking about, we do not have a theory, because we do not have a subject-matter. In this case, the theory is finitely axiomatisable, so that a consistency proof without constructing a model is unlikely; it is in any case a first-order theory, so that, by the completeness theorem, its consistency would entail the existence of a model. But, if an angel from heaven assured us of its consistency, we should still not have a mathematical theory until we attained a grasp of the structure of a model for it.

The status of the context principle

The GCP is surely incoherent. Indeed, it can scarcely be called a principle, since it embodies no criterion for distinguishing those terms whose reference is to be fixed by fixing that of more complex terms containing them from those whose reference is to be given outright. But since, in *Grundgesetze*, the only referents that Frege does specify outright are truth-values, the GCP there reduces in practice to the restricted context principle for reference. This cannot be construed as entitling us to ascribe any but the most nominal kind of referentiality to contextually defined terms; but since, in his mature period, Frege repudiated contextual definition altogether, this is not too great a concession.

We have, then, to restrict our considerations to abstract terms not understood by means of a method of transforming sentences containing them into sentences from which they are absent. For such terms, the restricted context principle, considered only under its general formulation, rather than in the light of the applications Frege makes of it, is scarcely open to question. Informal discourse is permeated by abstract terms. Here is a paragraph taken at random from the front page of a national daily newspaper:

Margaret Thatcher yesterday gave her starkest warning yet about the dangers of global warming caused by air pollution. But she did not announce any new policy to combat climate change and sea level rises, apart from a qualified commitment that Britain would stabilise its emissions of carbon dioxide – the most important

⁶ For a more detailed discussion of this matter, see Michael Dummett, 'Frege on the Consistency of Mathematical Theories', in M. Schirn (ed.), *Studien zu Frege/Studies on Frege*, vol. I, Stuttgart, 1976, pp. 229–42, reprinted in M. Dummett, *Frege and Other Philosophers*, Oxford, 1991, pp. 1–16.

'greenhouse' gas altering the climate – by the year 2005. Britain would only fulfil that commitment if other, unspecified nations promised similar restraint.

Save for 'Margaret Thatcher', 'air' and 'sea', there is not a noun or noun phrase in this paragraph incontrovertibly standing for or applying to a concrete object (is a nation a concrete object, or a gas?). Ordinary literate people readily understand such paragraphs; few would be easily able to render them in words involving reference only to concrete objects, if indeed they can be so rendered, or even to understand such a rendering if presented with it. An ordinary reader's comprehension of the abstract terms does not consist in the grasp of any such procedure of translation, but in a knowledge of how those terms function in sentences: no reason whatever exists for supposing him to attach a reference to 'Margaret Thatcher', but not to 'the climate' or 'air pollution'.

The notion of reference to an object is employed to mark the difference in linguistic function between a singular term and a predicate or relational expression; and that difference is as salient in the sentence 'Carbon dioxide is a compound' as in 'Margaret Thatcher is a woman'. One can know a great deal about Margaret Thatcher without ever having had to identify her; but, to understand a personal name, one has to know that there is such a thing as identifying its bearer. There being such a thing is what constitutes it as referring to its bearer, and explains our understanding of its use in predicating something of its bearer. Identification of a county, say as that in which one is, of a gas, say as being emitted from an exhaust pipe, of a political group, say as holding a meeting, all differ greatly from identifying a person, because counties, gases and political groups are things of very different kinds from people: but such identifications occur, and play the same fundamental role in our discourse about such things as the identification of people plays in our discourse about them. To deny to those things the status of objects, and to the corresponding expressions the function of referring to them, is to fall into nominalist superstition, based ultimately on the myth of the unmediated presentation of genuine concrete objects to the mind.

The context principle in mathematics

The language of the mathematical sciences differs markedly from that of everyday discourse: it could be said that the semantics of abstract terms bifurcates, according as we are concerned with one or the other. In the first place, terms capable of definition are likely to be introduced by definition. Terms not introduced by definition can therefore be expected to be indefinable within the framework adopted for the theory in question; and this makes it more problematic to understand how the senses of sentences containing them are fixed. In the second place, and more importantly, the concept of identification is harder to apply to mathematical objects than to abstract objects of

the kind referred to in informal discourse; and this renders it more difficult to justify the ascription of reference, in any full-blooded sense, to mathematical terms.

Crispin Wright and others are right to see the example of directions, used by Frege in *Grundlagen*, as having guided his thinking in this matter. Even though he had no use for the conception of reference as effected by a mental association between a term and its referent, considered as apprehended independently of language, he did allow a distinction between objects of which we could, and those of which we could not, have an intuition. Of a line, an intuition was possible; of a direction, it was not. The distinction corresponds, at least roughly, with that between an object that can be an object of ostension, and one that cannot. Hence the transition from speaking of lines to speaking of their directions could be effected only by coming to grasp the senses of sentences in which directions are referred to; and in such sentences, a direction could not be referred to directly by a demonstrative phrase, but only by a phrase picking it out as the direction of some line (possibly itself identified demonstratively). Having rightly perceived that the fundamental class of such sentences was that of statements of identity between directions, Frege leapt to the conclusion that the basis for introducing any new range of abstract terms must consist in the determination of the truth-conditions of identity-statements involving them. In a certain sense, this was not far from the truth. It led, however, to the root confusion that allowed him to believe that he could simultaneously fix the truth-conditions of such statements and the domain over which the individual variables were to range. This belief was a total illusion. To arrive at an interpretation of a formal language of the standard kind, employing an essentially Fregean syntax, we have *first* to attain a grasp of the intended domain of the individual variables: it is only after that that we can so much as ask after the meanings of the primitive non-logical symbols.

The confusion into which Frege fell did not invalidate the context principle as such; but it fatally vitiated his application of it. It is this confusion that constitutes the central flaw in his entire philosophy of arithmetic. It was in consequence of it that he believed that he had discovered an incontrovertible means whereby to fix the senses of all sentences of any precisely formulated mathematical theory (or at least of any not demanding appeal to intuition). All that was essentially needed, according to this conception, was, first, to fix the criterion of identity for the characteristic terms of the theory, and then to lay down a further criterion to determine whether any such term was to be taken as having the same reference as a term for an object of any other kind (such as a truth-value) to which the theory was required to allude. Given this, the domain of the theory was determined; the interpretation of other symbols of the theory would then be unproblematic, and the truth-values of all sentences of the theory would thereby be determined. By appeal to the context principle, one could then infer, from the determinateness of the truth-conditions of all

sentences containing terms for the objects of the theory, that those objects are given to us as the referents of those terms. In this manner, Frege thought, he had hit on a universally applicable method of justifying the assumption of the existence of logical objects, as the objects of a mathematical theory, of any range whatever. He had, however, fatally overlooked the circularity of the entire procedure: that of specifying the criterion of identity in terms of the truth of sentences of the theory, and, more generally, that of attempting simultaneously to specify the domain and the application of the primitive predicates to its elements.

Frege's discussion in *Grundlagen*, §§ 62–9, taught him, as he supposed, a second lesson: namely, that all logical objects, or at least all those needed in mathematics, could be defined by logical abstraction, except, of course, the classes needed for such definitions. In *Grundlagen*, the notion of the extension of a concept is introduced very tentatively: the idea of using it was evidently one that had only recently occurred to Frege. By the time *Grundgesetze* was written, it had taken firm root: the general problem how to justify the introduction of logical objects of any one kind had been reduced to the particular problem how to justify the introduction of classes, or, rather, of value-ranges. Thus Frege was not merely in possession of a general strategy for justifying the introduction of mathematical objects: he believed that, by applying that strategy to justify the introduction of value-ranges, he had justified the introduction of all of them, once for all.

Despite the lack of importance generally attached to modelling whole mathematical theories within set theory, we have not, in this latter regard, moved very far beyond Frege's viewpoint: definition in terms of equivalence classes has been adopted as a standard device, to be applied whenever available. In the main respect, however, that of the specification of the domain of a formal theory, our perspective is utterly different from Frege's. Nobody followed Frege in this matter; virtually none attempted even to understand him.⁷ We recognise no universal and unquestionable method of specifying the domain of a theory, but, on the contrary, acknowledge it as problematic how this is to be done and when it is possible to claim to grasp a domain. In the present context, a specification of a domain as consisting of objects presumed already known – the real numbers, for instance – is irrelevant: what matters is how it is to be specified when it is not taken as already known. Whether we have a formalised mathematical theory or merely an as yet uninterpreted formal system depends on whether we have some intuitive grasp of the structure of a model for the theory. The conception of the cumulative hierarchy, for instance,

⁷ Thus Hilbert, in 1904, merely criticised Frege for adopting a notion of set that led to the paradoxes, without bothering even to refute Frege's attempted emendation of his Axiom V: see D. Hilbert, 'Über die Grundlagen der Logik und der Arithmetik', *Verhandlungen des Dritten Internationalen Mathematiker-Kongresses in Heidelberg vom 8. bis 13. August 1904*, Leipzig, 1905, pp. 174–85, reprinted in J. van Heijenoort (ed.), *From Frege to Gödel*, Cambridge, Mass., 1967, pp. 129–38.

renders Zermelo-Fraenkel set theory a genuine mathematical theory, in contrast to Quine's New Foundations. Quite obviously, however, that conception does not give us a precise, but only a highly generic, idea of a model for the theory: it relies on the problematic notions of the power set and of the totality of ordinal numbers. If we think of the elements of a domain as individuated by being associated with the elements of some index set, then Zermelo-Fraenkel sets may be indexed by well-founded trees, in which no two or more nodes determining isomorphic subtrees lie immediately below the same node. Here, characteristically, while the notion has some effect in conveying an intuitive grasp of the structure described by the theory, the indices are not in general finitely representable, and the conception of the index set involves the same difficulties as that of the domain it indexes. Our grasp of what is meant by speaking of *all* such trees remains indeterminate in the same two respects: the height of the trees (where the height of a node is the smallest ordinal greater than the heights of the nodes below it), and the totality of trees of any given height. Set theory is, of course, the most problematic case, principally because an intuitively natural model for it must be of such enormously high cardinality (from the viewpoint of an ordinary Earth-dweller). The problematic cases are precisely those from which we derive our conception of that cardinality which the domain must intuitively have: any index set must have the same cardinality, and hence a challenge to our claim to grasp the domain can never be conclusively rebutted. This applies equally to the natural numbers, from which we derive our conception of a denumerable totality. No refutation can be devised to defeat, on his own ground, a finitist who professes not to understand the conception of any infinite totality: Frege was mistaken in supposing that there can be a proof that such a totality exists which must convince anyone capable of reasoning.

In any given case, the difficulty is to know what we ought to allow as sufficing to convey a determinate conception of a domain of mathematical objects: this is one of the principal sources of divergent practices within mathematics, as well as of disagreements in the philosophy of the subject. Some comments on this are reserved for the final chapter; but Frege evaded all such discussion because he falsely believed that he had a short cut taking him straight to the final goal. It remains of importance not just to dismiss his view as totally misconceived, but to pinpoint his error. The context principle allows us to ascribe a reference to mathematical terms provided that we have fixed the truth-conditions of sentences in which they occur; but Frege was completely mistaken about how we can go about fixing such truth-conditions. His mistake leaves us in perplexity about the content of the context principle: for, although his method of fixing the truth-conditions was not a contextual definition, it gives rise to exactly the same doubt about how it can justify the ascription of a genuine reference, robustly conceived, to the terms in question as we rightly feel concerning terms introduced by contextual definition. That is because no

actual use is made of the notion of reference in Frege's purported procedure for fixing the truth-conditions: he tells us that, when he has carried it out, the terms being introduced will have a reference, but the procedure makes no appeal to any relation between them and elements of the domain. As a result, we have no conception of any analogue of the notion of identifying an object as the referent of a term which plays a role in all other cases, abstract or concrete; the referents of the newly introduced terms cannot be thought of in any other way than simply as the referents of those terms, and hence the analogy with other cases, which ought to sustain all uses of the notion of reference, is here lacking.

The problem what constitutes a legitimate method of specifying the intended domain of a fundamental mathematical theory – one we do not treat as relating unproblematically to an already known totality of mathematical objects – remains intractable; Frege's philosophy of mathematics contributed precisely nothing to its solution, and is in that respect gravely defective. Certain ways of specifying the truth-conditions of the statements of a theory dispel its apparent ontological implications; in such cases, the context principle does not of course apply. This occurs when the statements of the theory are so interpreted that the terms they contain are not taken at face value as denoting elements of the domain; an example is the Kreisel-Troelstra interpretation of the intuitionistic theory of choice sequences, under which no reference to or quantification over choice sequences remains. To adopt such an interpretation is not, indeed, merely to admit that it yields a truth-preserving translation, but to treat it as giving the meanings of the statements it interprets. But, when the intended meanings of the statements of the theory are explained by first laying down what the domain comprises, and then interpreting the terms of the theory as denoting particular elements of that domain, Frege's context principle is entirely correct in pronouncing that there is no further problem of warranting the ontological implications of the theory: whether or not the purported explanation is legitimate may be problematic, but, if it is, there is no *further* problem. This is because any such explanation will necessarily embody some means of individuating particular elements of the domain, whether within the semantic account, by appeal to an index set or otherwise, or as the denotations of terms of the theory belonging to some canonical range. In contrast to Frege's procedure, such a method will supply an analogy to the intrinsically analogical notion of identifying an object as the referent of a term; since this notion plays a functional part in the interpretation, we are justified in ascribing a genuine reference, robustly understood, to the terms of the theory, which Frege's own procedure gave no acceptable grounds for doing.

The upshot of our prolonged enquiry into the validity of the context principle, considered as legitimating the use of terms for abstract objects, is then as follows.

The principle, as used in *Grundlagen*, really tells us no more than that the

use of such terms is legitimate if we have succeeded in assigning truth-conditions to sentences in which they occur. A nominalist would contest even this; but from no sober point of view can it be disputed. What must be disputed, however, is Frege's – and Wright's – idea of what is sufficient for determining the truth-conditions of sentences containing terms of a newly introduced kind. Impredicative specification of the conditions for the truth of identity-statements involving one or two such terms is *not* sufficient, contrary to Frege's belief and to that of his disciple Crispin Wright. It fails to fix truth-conditions for all sentences containing the new terms, when these terms are formed by attaching an operator to a predicate or functional expression; and it fails to do so because of the lack of an independent specification of the domain, which it attempts, but fails, to circumscribe simultaneously with its determination of the truth-conditions of sentences containing the new terms.

When the context principle is construed, not merely as legitimating the use of abstract terms, but as justifying the ascription of reference to them, the question has substance only when the notion of reference is understood as belonging to a semantic theory for the language as a whole. In this case, the foregoing remarks about the method adopted by Frege and favoured by Wright continue to apply: the procedure now attempts, but fails, simultaneously to circumscribe the domain and to determine the references of the new terms. How, then, would it be if the new terms were introduced, not by a circular procedure of that kind, but by a genuine contextual definition? In such a case, no view stronger than an intermediate one could be taken of a claim that a reference had thereby been conferred upon them; the reference so conferred would be reference only in the thin sense of *Grundlagen*, since the notion would play no role in the semantic account of how the truth-values of sentences containing the terms are determined.

The retort might be made that there are not two notions of reference, a thin one and a substantial one: there is only one notion, the thin one according to which 'the direction of *a*' refers to the direction of *a*, whether or not the direction-operator has been explained by means of a contextual definition. The illusion that any more robust notion exists arises, according to this reply, from linking the theory of reference too tightly to the theory of sense. The reference of a singular term, like that of any other expression, is its contribution to the determination of the truth-value of a sentence in which it occurs, in virtue of how things are. It need have nothing to do with *our* means of coming to recognise the truth-value of such a sentence, which indeed depends upon our grasp of its sense. If the sense of the term has been given to us by means of a contextual definition, then, admittedly, our route to recognising the sentence as true may go through an initial transformation of the sentence, in accordance with the contextual definition, into another sentence in which the term does not occur; but this has nothing to do with how the sentence is objectively determined as true, as this is explained by the theory of reference.

This reply misconstrues Frege's conception of the relation between the notions of sense and of reference, which *are* indissolubly linked on his understanding of them. The theory of sense rests upon the theory of reference as a base; and Frege was right to conceive of them as so related. A semantic theory is not justified solely by its according the right truth-values to the sentences of the language, in the light of the way things are: it has also to be adapted to serve as a base for a correct theory of sense. In the celebrated § 32 of *Grundgesetze*, Vol. I, Frege referred to his preceding stipulations of what the references of the expressions of his formal language were to be, and then said:

Every such name of a truth-value *expresses* a sense, a thought. That is, it is determined by our stipulations under which conditions it refers to the True. The sense of this name, the *thought*, is the thought that these conditions are fulfilled.

It therefore matters how the stipulations concerning reference were formulated: they needed to be framed in such a way that a grasp of them would yield the intended sense. The passage can be read as saying that sense cannot be stated, but only shown by the manner in which the reference is stated. We do not need to draw so strong a conclusion from it: it is consistent with holding that it is possible to state informatively what is required for a grasp of the sense of a given expression. But it certainly requires that the sense must be shown by the way the reference is laid down in the semantic theory. The *point* of a semantic theory, and what is required for it to be a correct theory, is that it should be capable of serving as a base for a correct theory of sense.

The conception of a semantic theory – in Frege's case, his theory of *Bedeutung* – as describing the mechanism whereby the truth-values of sentences are determined is most easily understood when it can be taken as relating to the canonical means by which *we* decide their truth or falsity. A realistic theory like Frege's, however, cannot be understood in this fashion; it must nevertheless be taken as reflecting our grasp of the truth-conditions of sentences. The notions of sense and of reference are thus in symbiosis: the semantic theory is a base for the theory of sense, but must for that very reason be constructed with an eye to its role as such a base. A realistic semantic theory must thus be understood as embodying our conception of how the truth-values of our sentences are determined by the way things are. This becomes obvious when it is put by saying that our grasp of the thought expressed by a sentence involves a grasp of its semantic structure: to know the sense of each component expression – that is, of each semantically significant component – we must know its logical type, and hence the type of thing to which it refers; and to grasp the thought expressed by the whole, we must understand how these components are related to one another in the sentence in such a way that their referents together determine it as true or as false. A grasp of the sense of the sentence thus comprises, but is not exhausted by, an

understanding of how its truth-value is determined in accordance with its structure, as this is explained in the theory of reference.

We should distinguish between what the reference of an expression *is*, which is independent of how it is given to us, and how the theory of reference needs to state what its reference is, if it is to accord with Frege's implicit requirement, in § 32 of *Grundgesetze*, Vol. I, that the stipulation of its reference should show what its sense is. If an expression is introduced by an explicit definition, or is taken to be understood by a tacit appeal to that definition, then its reference is whatever is the reference of the defining expression, because that is what the definition lays down. The referent is then given to us as the referent of the defining expression; and so, in order to show what the sense of the defined expression is, the semantic theory (theory of reference) must stipulate its reference precisely by means of that definition: the definition must be incorporated into the theory of reference, and not just into the theory of sense.

How, then, does it stand with terms introduced by contextual definition? In the theory of *Grundgesetze*, the question does not arise, because contextual definition is rejected as an illicit procedure. If, however, it were admitted, then, by the same token, the contextual definition would have to figure within the theory of reference; for, by hypothesis, our conception of how the truth-value of a sentence containing a term so defined would go via a transformation of the sentence which deleted (rather than replaced) the term. The theory of reference would therefore assign no reference to the contextually defined term, but only a truth-value to sentences containing it; and this would correspond to the fact that determining the referent of the term would play no part in the determination of the truth-value of such a sentence, as we conceived of this.

But would this not conflict with the generalised context principle of *Grundgesetze*? Could we not construe the contextual definition, not as denying a reference to the term, but as fixing its reference by determining the values of various functions for its referent as argument? We might try replying that the latter interpretation would be in place only if the semantic theory *required* us to ascribe a reference to the term, and other terms of the same form, and that this would happen only when the domain of quantification could be grasped only as comprising the referents of those terms; we could add that, in such a case, we could not have a contextual *definition*, but only a contextual stipulation within the semantic theory, probably one of the misbegotten variety employed by Frege. It is better simply to acknowledge that, if Frege had admitted contextual definitions, there would have been a conflict, but to blame this on the incoherent character of the GCP. Indeed, within the theory of reference of *Grundgesetze*, the context principle for reference would be incoherent even when the relevant contexts were restricted to *sentences*. When the notion of reference is the instrument of a serious semantic theory, serving as the base for a theory of sense, the context principle simply cannot be sustained in full generality; against that background, it is useless to mount a defence of it.

The notion of reference, as applied to singular terms, is operative within a semantic theory, rather than semantically idle, just in case the identification of its referent is conceived as an ingredient in the process of determining the truth-value of a sentence in which it occurs. Hence the context principle, if it is to warrant an ascription of reference to a term, robustly understood, must include a further condition if it is to be valid. It is not enough that truth-conditions should have been assigned, in some manner or other, to all sentences containing the term: it is necessary also that they should have been specified in such a way as to admit a suitable notion of identifying the referent of the term as playing a role in the determination of the truth-value of a sentence containing it. With that further condition, the context principle ceases to be incoherent, and gains the cogency Frege took it to have: it will then no longer give even the appearance of validating the means of introducing value-range terms which led Frege into contradiction, the analogue of which for numerical terms Wright seeks to defend.

The additional condition will probably always be met by appropriate explanations – not amounting to contextual definitions – of terms for dependent abstract objects: those objects, like the Equator, whose existence is contingent upon the existence and behaviour of concrete objects. It is probably also the case for all legitimate means of specifying the domains of mathematical theories, the existence of whose elements is required by the mathematical character of the theories to be independent of any matters of contingent fact. We must here leave in abeyance the question whether there in fact exist legitimate, non-circular methods of specifying the domains of the fundamental theories of classical mathematics. Frege never advanced the context principle as having the advantages conceded by Russell to the method of postulation: it merely indicated *what* honest toil was called for. It was his error and his misfortune then to have misconstrued the task, an error for which he paid with the frustration of his life's ambition.

The discussion has here been conducted throughout as if the distinction between concrete and abstract objects, or Frege's corresponding distinction between actual and non-actual objects, were a sharp dichotomy, as it is usually assumed to be. In fact, it is nothing of the kind, but rather resembles a scale upon which objects of varying sorts occupy a range of positions. The criterion of causal efficacy cannot be unequivocally applied in all cases: Frege himself fell into difficulties over it, in 'Der Gedanke', concerning thoughts, which he wanted to classify as non-actual, but could not deny some influence upon events; for, if someone judges some thought to be true, that may well affect his actions. This hardly bears upon the assessment of the context principle, however, especially in application to mathematical objects, which, on any account, occupy the extreme abstract end of the scale.

The context principle, understood as including the further condition, rules out all grounds for cavil at construing mathematical theories as having abstract

objects for their subject-matter. Proposals to treat such objects as symbols, as mental constructions, as fictions, as creations of the human mind, are shown by it to rest upon coarse misunderstandings of the functioning of our language: as Frege held, they are given *in* thought, but not created *by* thought. When we have accepted this, we see how short a distance we have advanced. The real problems of the philosophy of mathematics are far more specific: the existence of abstract objects was never more than a pseudo-problem, and, when we have recognised it as such, the real problems remain. Great credit is due to Frege for enunciating the principle which exposes it as a pseudo-problem, to which many other philosophers of mathematics have laboured to produce pseudo-solutions. This credit ought not to be denied on the ground of his mistake in applying his own principles, and so in effect treating it as providing too easy a solution to the real problems, as well as a definitive answer to the spurious ones; he suffered sorely enough for that mistake, after all.

Part III of Grundgesetze

As already remarked, the division of *Grundgesetze* into volumes bears no relation to the architecture of the book. Volume I contains Part I and most of Part II, Part I being concerned with the syntax and semantics of the formal system, and Part II with the entirely formal presentation of Frege's foundations for the theory of natural numbers, together with the smallest transfinite cardinal. Volume II, published ten years later, contains the remainder of Part II, and about three-quarters of Part III, entitled 'The real numbers', together with the Appendix on the Russell paradox.¹ Frege obviously intended a third volume; the last words of the main text (§ 245) read 'The next problem will now be to show that there exists a positive class, as indicated in § 164. The possibility will thereby be opened of defining a real number as a ratio of quantities of a domain belonging to a positive class. And we shall then also be able to prove that the real numbers are themselves quantities belonging to the domain of a positive class.' If Volume III had contained only the conclusion

¹ It seems likely that most of vol. II was already written in 1893, or shortly afterwards. Most of Frege's references are to works published before that year. Of his own writings, the only exceptions are his letter to Peano of 1896, published in the *Rivista di matematica* for that year, and his *Über die Zahlen des Herrn H. Schubert* of 1899; both are cited in brief footnotes (to §§ 65 and 153) that could easily have been added subsequently. Frege cites only three works from after 1893 by other authors: Peano's reply to his letter, published in the same issue of the *Rivista*; J. Thomae's *Elementare Theorie der analytischen Functionen einer complexen Veränderlichen* (Halle) in its second edition of 1898; and Alfred Pringsheim's article 'Irrationalzahlen und Konvergenz unendlicher Prozesse' in the *Encyklopädie der mathematischen Wissenschaften*, vol. I, pp. 47–146, originally published (together with Schubert's article) in Heft 1, issued in 1898. A long footnote to § 58 discusses Peano's reply, and may well also have been added later. Thomae's book is discussed and quoted from at great length; but the first edition, containing all the cited passages, had been published in 1880. Pringsheim's article is discussed briefly in § 72; its omission would not impair the continuity of the section of which it forms part.

Two fragments of Frege's Nachlass obviously meant for inclusion in vol. II of *Grundgesetze*, one concerned with principles of definition, the other with the notion of a variable, tell against the hypothesis (*Nachgelassene Schriften*, pp. 164–81, *Posthumous Writings*, pp. 152–66). The former discusses the definition of the implication sign given in Peano's *Formulaire de mathématique*, vol. 2 (1897), and the latter refers to a book of E. Czuber published in 1898. Neither fragment found its way into the book. The discussion of definition in §§ 55–67 corresponds very little to the first fragment; and Frege in fact treated the second topic in his 'Was ist eine Function?' of 1904. Probably these were rejected additions to a text already substantially complete.

of Part III, it would have been extremely short. Possibly Frege had in mind a Part IV, dealing with complex numbers. It may be thought that that would have been pointless, since it is easy to define the complex numbers in terms of the reals; but Frege was much concerned with applications, and the applications of complex analysis are by no means immediately evident from the representation of complex numbers as ordered pairs of reals, or even from the geometrical interpretation, which in any case he would have disliked as involving the intrusion into arithmetic of something dependent upon intuition.² However this may be, what we have is an uncompleted Part III, largely neglected even by Frege's admirers.³

Unlike Part II, Part III is divided into two halves: III.1, entitled 'Critique of theories of irrational numbers', is in prose, while the uncompleted III.2, entitled 'Theory of quantity', is occupied, like the whole of Part II, with formal proofs and definitions. In III.1, Frege attempted to do for real numbers what he had done for natural numbers in *Grundlagen*, §§ 5–54. The same general plan is followed. Existing theories are surveyed and subjected to criticism; a synopsis (§§ 156–9) is devoted to drawing the moral from the failure of the various theories so reviewed; and a brief concluding section (§§ 160–4) sketches the theory Frege intends to put in their place.

Unhappily, the attempt woefully miscarried. The critical sections of *Grundlagen* follow one another in a logical sequence; each is devoted to a question concerning arithmetic and the natural numbers, and other writers are cited only when either some view they express or the refutation of their errors

² See *Grundlagen*, § 103; also § 19.

³ In 1913, Philip Jourdain wrote an impertinent and monumentally tactless letter to Frege, saying, 'In your last letter to me you spoke about working at the theory of irrational numbers. Do you mean that you are writing a third volume of the *Grundgesetze der Arithmetik*? Wittgenstein and I were rather disturbed to think that you might be doing so, because the theory of irrational numbers – unless you have got a quite new theory of them – would seem to require that the contradiction has been previously avoided; and the part dealing with irrational numbers on the new basis has been splendidly worked out by Russell and Whitehead in their *Principia Mathematica*.' Jourdain had obviously never looked at vol. II of *Grundgesetze*, or he would have realised that Frege had already gone far towards expounding his own theory of real numbers.

It is possible that the work to which Frege had been referring in the letter to which Jourdain was replying was that contained in the manuscript entitled 'Das Irrationale, gegründet auf Anzahlklassen', irretrievably lost through American bombing and Heinrich Scholz's failure to make a copy of it; see Albert Veraart, 'Geschichte des wissenschaftlichen Nachlasses Gottlob Freges und seiner Edition', in Matthias Schirn (ed.), *Studien zu Frege/Studies on Frege*, vol. I, Stuttgart, 1976, p. 98, no. 76.

The only modern studies of Frege's theory of real numbers are: Franz Kutschera, 'Freges Begründung der Analysis', *Archiv für mathematische Logik und Grundlagenforschung*, vol. 9, 1966, pp. 102–11, reprinted in M. Schirn (ed.), op. cit., pp. 301–12; G. Currie, *Frege: an Introduction to his Philosophy*, Brighton, 1982, pp. 57–9; idem, 'Continuity and Change in Frege's Philosophy of Mathematics', in L. Haaparanta and J. Hintikka (eds.), *Frege Synthesized*, Dordrecht, 1986, pp. 345–73; S.A. Adeleke, M.A.E. Dummett and Peter M. Neumann, 'On a Question of Frege's about Right-Ordered Groups', *Bulletin of the London Mathematical Society*, vol. 19, 1987, pp. 513–21; and Peter M. Simons, 'Frege's Theory of Real Numbers', *History and Philosophy of Logic*, vol. 8, 1987, pp. 25–44.

contributes positively to answering the question. In Part III.1 of *Grundgesetze*, the sections follow no logical sequence. Each after the first, which concerns general principles of definition, is devoted to a particular rival mathematician or group of mathematicians: (b) to Cantor, (c) to Heine and Thomaë, (d) to Dedekind, Hankel and Stolz, and (e) to Weierstrass. From their content, the reader cannot but think that Frege is anxious to direct at his competitors any criticism to which they lay themselves open, regardless of whether it advances his argument or not. He acknowledges no merit in the work of those he criticises; nor, with the exceptions only of Newton and Gauss, is anyone quoted with approbation. The Frege who wrote Volume II of *Grundgesetze* was a very different man from the Frege who had written *Grundlagen*: an embittered man whose concern to give a convincing exposition of his theory of the foundations of analysis was repeatedly overpowered by his desire for revenge on those who had ignored or failed to understand his work. The consequence is that the reader is not directed, as in *Grundlagen*, along a path appearing to lead irresistibly to Frege's own theory as the only surviving possibility. Instead, he finishes the critical sections so wearied by the relentless carping at every detail of the theories examined – almost always warranted, but never generous and frequently irrelevant – that he has very little idea what fundamental objection Frege has to them.

Weierstrass

The most lamentable example is the last critical section (e), on Weierstrass. Leaving his great contributions to the foundations of analysis unmentioned, it descends rapidly into the grossest abuse.⁴ Frege's criticism is justified, but the tone is unforgivable, directed as it is at a great mathematician; and since it is aimed at what Weierstrass says about the natural numbers, it is completely out of place.

The section serves no purpose but to advertise the ill manners Frege had acquired. No reader could have gone on from it convinced that the theory of real numbers to be expounded was the only one remaining in the field; none could view the five first sections of Part III.1 as presenting any ordered train of reasoning, but only as attacking all Frege's rivals in no particular sequence. Unless replaced by a serious examination of Weierstrass's theory of real numbers – essentially superseded by that of Cantor – the section on him ought to have been deleted in its entirety.

⁴ 'If a man who had never thought about the subject in his life were woken from sleep with the question, "What is number?", he would in his first confusion give voice to expressions similar to those of Weierstrass', Frege wrote in § 149, and was so pleased with the remark that he repeated it years later in his lecture series 'Logik in der Mathematik' (*Nachgelassene Schriften*, pp. 238–9, *Posthumous Writings*, p. 221).

Principles of definition

Even had the section on Weierstrass been deleted, the impact of Part III.1 would still have been muffled by its disordered arrangement. It might well be thought that the first section (a), entitled 'Fundamental principles of definition', was quite out of place. It is written as if it were an afterthought that should have been included in Part I of the book; but this is not wholly so. The first fundamental principle, to which Frege devotes §§ 56–65, is the principle of completeness; the second, that of simplicity of the expression defined, occupies only § 66. The principle of simplicity rules out contextual definition. That of completeness rules out piecemeal definition; the practice that occurs, as Frege expresses it (§ 58), 'when a symbol is first defined for a restricted domain, and then used in order to define the same symbol once more for a wider domain'; for example (§ 57), when 'the definition is given . . . for the positive integers . . . and after many theorems a second definition follows . . . for the negative integers and 0'. In a footnote to § 58, Frege cites Peano as endorsing the procedure and declaring it indispensable.⁵

It is this procedure which is relevant to the main topic, how the real numbers should be introduced. The most natural way to think of the introduction of negative integers, fractions, irrationals and finally complex numbers is as successive additions to already given number-systems. In this case, as the number-system is extended, the arithmetical operations of addition, multiplication and exponentiation must be extended, too: defined originally for non-negative integers, they must be extended by new definitions, first to the negative integers, then to fractions, then to irrational numbers and eventually to complex ones. It was to this conception that Russell repeatedly objected in his *Introduction to Mathematical Philosophy*. According to Russell, each new number-system, considered as subject to the basic arithmetical operations, contains a subsystem isomorphic to the previous one but *not* identical to it: the complex number $1 + 0i$ is a distinct object from the real number 1; the rational $1/1$ is not to be identified with the integer $+1$, nor that again with the natural number 1.

Frege shared Russell's opposition to the conception of the successive extension of the number-system: that is why he is careful to use different symbols for the natural numbers 0 and 1, which were for him cardinal numbers, and for the real numbers 0 and 1, writing the numerals for the natural numbers with slashes through them. He was well aware that, when a new number-system is introduced – that of the real numbers or of the complex numbers – the arithmetical operations have to be specifically defined for them, and that,

⁵ Frege quotes Peano as saying, in his reply to Frege's letter, 'If what is to be defined contains variable letters, and thus is a function of those letters, it appears to me necessary to give conditional definitions of that expression – definitions with hypotheses – and to give as many definitions as there are kinds of entities on which we carry out that operation. Thus the symbol $a + b$ will first be defined when a and b are integers, then a second time when they are fractions, and then when they are irrational or complex numbers.'

provided that they satisfy certain laws which we may lay down in advance as ones we want them to satisfy, there may be some latitude in the precise form of the definition. He believed, however, that each operation should be defined once and once only, and that the definition should cover all cases, so that the operation is defined for all objects whatever, and not just those to which we are interested in applying it; an incomplete definition will allow us to form terms for which no reference has been provided. Moreover, 'if we have no final definitions, we likewise have no final theorems' (§ 61). If a partial definition of multiplication may come subsequently to be supplemented by a further definition relating to a new, or wider, domain, we cannot affirm with assurance that 9 has only two square roots: for we cannot know that multiplication may not be defined over some new domain in such a way that for some element t of it, distinct from $+3$ and -3 , we shall have $t.t = 9$.

Frege's opposition to piecemeal definition thus serves as a ground for rejecting the conception, common in mathematical textbooks of his time, of the introduction of new sorts of number as the extension of an existing number-system by the adjunction of new elements; so regarded, it is highly pertinent to the main topic, and may well stand as the first section of his critique. It does not, however, supply very strong grounds for rejecting the conception in question; and Frege was aware that it did not. Certainly he was right that operations applicable within distinct domains are distinct operations, and should in principle be denoted by distinct symbols. But, even if we grant that it should be impossible to form terms lacking a reference, and even if we also grant that all functions should be defined for all objects as arguments, Frege could perfectly well have handled the process of extending a given number-domain had he believed that it provided the right framework for the transition from natural numbers to real numbers. He was forced to represent functions defined over restricted domains as relations, or, more usually, as the extensions of such relations, and employed the device repeatedly in *Grundgesetze*. Given the domain of natural numbers, it would be easy to define a ternary relation $A(n, m, k)$ as holding only between natural numbers and as obtaining just when $n + m = k$; the symbol $+$ could then be defined by means of the description operator. If, say, the signed integers were thought of as resulting from adjoining the negative integers to the domain of natural numbers, a new ternary relation, including the former one, could then be defined over the signed integers, and a new symbol for addition defined in terms of it: the procedure parallels those Frege does employ, and could not be objected to. He shows himself conscious that piecemeal definition can readily be avoided without any fundamental change in the structure of the domains to which the operation so defined is applied when he remarks (§ 60) that 'it is easy to avoid a plurality of definitions for one and the same symbol. Instead of first defining it over a restricted domain, and then using it for the purpose of defining itself over a wider domain – instead, therefore, of defining the same thing twice – we need only

choose different symbols, and restrict the reference of the first, definitively, to the narrower domain.'

It may have been because Frege realised that the rejection of piecemeal definition had, in itself, few substantial consequences that section (a) is not presented as having much bearing on the main topic; it could nevertheless have been made to appear a relevant opening to the review of prevailing theories about the introduction of real numbers if the short exposition of the principle of simplicity had been placed first, and the longer passage about the principle of completeness more explicitly addressed to the problem of introducing new kinds of numbers.

Frege had in fact a much stronger ground for opposing the conception of introduction as extension than his objection to piecemeal definition. His theory of real numbers differs from all others – from those of Weierstrass, Cantor, Dedekind and Russell – in omitting the intermediate steps of introducing the rationals and even of the signed integers: he goes straight from the natural numbers to the real numbers, positive and negative, without first defining any domain less extensive than the reals but more extensive than the natural numbers. His reason for doing so is that he identifies kinds of number by their applications: and, in the two published volumes of *Grundgesetze* (where complex numbers are not mentioned), he acknowledges only two kinds. Having remarked in § 157 of Volume II that 'we have interpreted real numbers as ratios of quantities', he goes on to say:

Since cardinal numbers (*Anzahlen*) are not ratios, we have to distinguish them from the positive whole numbers. It is therefore not possible to extend the domain of cardinal numbers to that of the real numbers; they are completely disjoint domains. The cardinal numbers answer the question, 'How many objects of a given kind are there?', whereas the real numbers can be regarded as measurement-numbers, which state how large a quantity is as compared with a unit quantity.

While Frege makes clear, in this as in other passages, the ground of differentiation between natural numbers and real numbers, he rather oddly never makes quite explicit the reason why his practice diverges from others in not recognising a distinct domain of rationals or even of integers. It is apparent, however, from his section (b) on Cantor that he saw the rationals, at least, as principally serving to answer the question, 'How great?', that is, as giving the magnitude of a quantity relatively to a unit quantity, and therefore as not warranting recognition as forming a domain distinct from that of the real numbers. A preliminary distinction between the questions, 'How great?' and 'How many?', could have been appended to section (a): this would have oriented the reader in the direction in which Frege wished him to go.

Postulation and creation

Section (b), a serious discussion of Cantor's theory of real numbers, is completely misplaced. It contains matter far more informative about the grounds for Frege's own theory than any other of the critical sections, and its proper place is at the end of the review of rival theories, not towards the beginning. The next section (c) is very long, and constitutes a brilliant examination of formalism as a philosophy of mathematics, the only one Frege ever undertook. It earns its place in a survey of theories of real numbers in part from the fact that one of his criticisms of formalism is that it is incapable of handling the concept of an infinite sequence. It does much more than that, however: it is the one passage in Frege's writings in which he makes explicit his views on the application of mathematics. It thus has an importance independent of its context; but it is also crucial to its context, since the motivation for Frege's theory of real numbers cannot be understood save in the light of his view of application. Since his criticism of Cantor turns on precisely this, the placing of section (c) after section (b) instead of before it was a serious error of arrangement.

In §§ 140–7 of section (d), Frege engages in a critique of the method of 'postulating' the existence of whatever new numbers – negative, rational, etc. – are needed at a given stage. This critique closely resembles that contained in the brief section of *Grundlagen* (§§ 92–104) on 'Other numbers'. In both, Hankel is used as an example of what Frege is criticising; in *Grundgesetze*, Otto Stolz serves as a second example. It is in place in a general critique of methods of introducing the real numbers, since such postulation was a favourite device among mathematicians of the day for proceeding from the positive integers to more extensive number-systems. Like most of the other sections, however, it is out of sequence. Since it tells us less about Frege's positive views than the critique of formalism, it ought to come second, after section (a) and before section (c).

The objections Frege raised to postulationism in *Grundlagen* were in line with Russell's famous remark that 'the method of "postulating" what we want has many advantages; they are the same as the advantages of theft over honest toil'.⁶ We cannot 'proceed as if mere postulation were its own fulfilment':⁷ we have to show that there *is* a system of numbers, with operations of addition and multiplication suitably defined upon them, satisfying the conditions we desire. If we were content to do no more than postulate such a system, we could not be sure that its existence did not contain a hidden contradiction; postulation therefore demands at least a proof of the consistency of what is postulated. However, the only way to establish consistency, according to Frege, is to prove that a system of that kind does exist; postulation thereupon loses

⁶ B. Russell, *Introduction to Mathematical Philosophy*, London, 1919, p. 71.

⁷ *Grundlagen*, § 102.

all its point. In any case, even if we could prove the consistency of the hypothesis in some other way, it would not give us what we need. Frege invokes his favourite example of de Moivre's theorem as a case in which theorems about real numbers can be deduced from one about complex numbers.⁸ For us to have the right to regard the theorems about real numbers as true, it is not enough that the hypothesis that complex numbers exist should not be self-contradictory, and that, if they existed, de Moivre's theorem would hold good of them: we need to know that they *do* exist. To prove that the rational, real and complex numbers exist, Frege says, we shall have to proceed as we did for cardinal numbers. We must first fix the criterion of identity for the numbers to be introduced; we shall then be able to define them, as before, as extensions of concepts.⁹

The discussion in *Grundgesetze* follows very similar lines; essentially all the same points are made. Although in *Grundlagen* Frege had called those who engage in the method of postulation 'formalists', he here recognises that the formalism he attacked in section (c) is a much more radical doctrine: the postulationists do not deny that mathematical symbols serve to denote objects distinct from themselves (§ 145). Instead of the example from de Moivre's theorem, Frege considers a case in which a theorem is proved by appeal to an auxiliary object not mentioned in the theorem but belonging to the same domain: specifically, a natural number invoked in a proof in number theory. To prove that, if p is prime, the congruences $x^n \equiv 1$ and $x^\delta \equiv 1 \pmod{p}$ have the same roots, where δ is the greatest common divisor of n and $p - 1$, we need to appeal to a primitive root of p .¹⁰ It is not sufficient, for the sake of the proof, to *postulate* that a primitive root exists: we need to *prove* that it exists (§ 140). As Frege remarks, the case is similar to that of the 'construction' of an auxiliary line in a geometrical proof.

The example is an instance of Fregean overkill. No postulationist, however brash, ever supposed that he might, in number theory, postulate the existence of a number, or, in geometry, of a line, that he needed for a proof: only whole systems of mathematical objects – particularly number-systems obtained by adjoining elements to an existing one – are postulated in this manner. Frege's point is, of course, that there is no essential difference between the procedures; but he makes it by affecting to suppose that the postulationists themselves see no difference.

The discussion in *Grundgesetze* diverges from that in *Grundlagen* in two notable respects. In *Grundlagen*, Frege speaks principally of 'postulation'. But, if postulation is its own fulfilment, the mathematician possesses a creative

⁸ *Grundlagen*, § 97.

⁹ *Grundlagen*, § 104.

¹⁰ By Fermat's theorem, if p is prime and does not divide a , $a^{p-1} \equiv 1 \pmod{p}$. When $p - 1$ is the smallest number d such that $a^d \equiv 1 \pmod{p}$, a is called a primitive root of p . Every odd prime has primitive roots.

power equal, within its realm, to God's. It was by the word of his mouth that the heavens were made; God had only to *say*, 'Let there be light', and there was light. So, likewise, for the postulationist, a mathematician has only to say, 'Let there be a square root of 2, or of -1 ', and there is one. So, throughout section (d), Frege speaks, not of postulation, but of creation (*Schaffen* or *Schöpfung*), although he does use the former of the German terms in *Grundlagen* also. Secondly, he has acquired a more sophisticated view of the whole matter. In *Grundlagen*, he had no doubts about the possibility of proving the existence of any mathematical system we need; for, as yet, he took for granted the availability of extensions of concepts as logical objects familiar to all. In *Grundgesetze*, all mathematical objects were indeed to be defined as value-ranges: but he now recognised that the existence of value-ranges themselves required justification, and could not be taken for granted. Their existence could not be proved by defining them as something else; and so, in §§ 146–7, he faces the question whether his own method of introducing them was not an instance of postulation or of mathematical creation. He asserts that it is not, or, at any rate, that 'it is quite different from the lawless, arbitrary creation of numbers by many mathematicians'. Without the means provided by value-ranges, 'a scientific foundation for arithmetic would be impossible', he says; it serves to attain 'the ends that other mathematicians mean to achieve by the creation of new numbers'. These two paragraphs suggest an uneasiness that was to be proved well founded.

Dedekind

The first two paragraphs of section (d) are devoted to Dedekind's theory of real numbers. The first, § 138, acknowledges that he is no formalist, in the sense of Heine and Thomae; so the critical discussion is confined to § 139. Frege briefly describes the celebrated idea of a cut in the rational line. He then quotes Dedekind as saying:¹¹

Now whenever a cut (A_1, A_2) occurs which is not generated by a rational number, we *construct* (*erschaffen*) a new *irrational* number which we regard as completely defined by the cut (A_1, A_2) ; we shall say that the number α corresponds to this cut, or that it generates this cut.

Frege attacks this as an instance of mathematical creation; leaving Dedekind behind, he first gives his example of the primitive roots of p , and then launches on his critique of Hankel and of Stolz.

Frege was unquestionably right to criticise Dedekind for resorting at this point to 'construction'. Russell independently made the very same criticism:¹²

¹¹ R. Dedekind, *Stetigkeit und irrationale Zahlen*, Brunswick, 1872, § 4, 'Creation of the Irrational Numbers', pp. 14–15. The italics are Dedekind's.

¹² B. Russell, *Principles of Mathematics*, London, 1903, § 267.

What right have we to assume the existence of such numbers? What reason have we for supposing that there must be a position between two classes of which one is wholly to the right of the other, and of which one has no minimum and the other no maximum?

A mathematician has no right to announce that he will construct a range of new mathematical objects satisfying certain conditions unless he provides, or can assume known, a method of defining them accordingly.

As Russell saw, however, Dedekind's appeal to construction is unnecessary: the real numbers, rational and irrational, can be identified with the cuts themselves, or rather, with, say, those whose lower class contains no greatest element (or, more simply, with the lower classes of such cuts). The case, as Frege ought to have recognised, is quite different from one in which a mathematician postulates a system of numbers satisfying certain general conditions. Dedekind had provided a totality, composed of classes of rationals, with which the real numbers could be correlated one to one; he had done all the honest toil required. Frege could have perceived the possibility of emending the theory so as to dispense with the need for any construction as well as Russell did. In his review of Cantor, after objecting to Cantor's introduction of order-types by psychological abstraction, he commented that 'what Herr Cantor aims to get hold of can be unobjectionably defined'; had he chosen, he could have said the same of Dedekind. Dedekind's resort to construction was not a means of avoiding labour. It was due solely to his philosophical orientation, according to which mathematical entities are to be displayed as creations of the human mind.

We rightly think of Dedekind's as one of the leading theories of real numbers. Frege's curt dismissal of it undermines the claim of Part III.1 of *Grundgesetze* to survey the range of available theories of the real numbers on offer, and was a discourtesy to his readers as well as to Dedekind. To the former he owed it to provide a serious ground for rejecting Dedekind's theory, suitably emended, instead of a captious one fastening on an inessential detail. The fact pointed out above, that both Cantor's and Dedekind's theories were unsatisfactory from Frege's standpoint, in that they depended upon a prior definition of the rationals which Frege, for arguable reasons, declined to see as composing an independent number-system, would have provided one such ground, that Frege could have expounded had he chosen. The two paragraphs on Dedekind, together with the whole section on Weierstrass, illustrate how gravely Frege's embitterment impaired his ability to emulate in Part III of *Grundgesetze* the great achievement of *Grundlagen*.

An emendation of Part III.1 would then have started with a slightly revised section (a), followed by section (d) without the two paragraphs about Dedekind. After this would have come section (c) on formalism. Ideally, this would have been followed by serious studies of the theories of Dedekind and of Weier-

strass, the whole concluded by section (b) on Cantor's theory. Such a version might have been worthy to stand comparison with *Grundlagen*: at least it would not have left the bad taste in the mouth that is one of the effects of Part III.1 as it now stands.

CHAPTER 20

The Critique of Formalism

Section (c) of *Grundgesetze*, Part III.1, running from § 86 to § 137, is occupied with a critique of radical formalism, as represented by Frege's colleague at Jena, Johannes Thomae, and by E. Heine. Radical formalism is the doctrine that the formulae of a mathematical theory do not express genuine statements that can be true or false, but are merely uninterpreted strings of figures – letters in an alphabet, in the generalised sense – which the mathematician manipulates according to prescribed rules. Radical formalism may be propounded as a local or as a global thesis: that is, as applying to some one or more mathematical theories, independently of what holds good about others, or as one holding good, of necessity, of *all* mathematical theories. This section of *Grundgesetze* is the only passage in Frege's writings in which he offers a critique of radical formalism.

He offers three main objections to it. They are:

- (1) that it cannot account for the application of mathematics;
- (2) that it confuses a formal theory with its metatheory;
- (3) that it can give no coherent explanation of the concept of an infinite sequence.

Infinite sequences

Of Frege's three objections to formalism, the third, developed in §§ 121–36, is directly relevant to the theory of real numbers; it is made apropos of the introduction of irrational numbers into formalist arithmetic, which, as Frege says in § 124, is effected by both Heine and Thomae in a manner superficially resembling that of Cantor, by means of infinite sequences of rationals satisfying the Cauchy condition for convergence. But the fact that the stipulations have to be understood in the light of the governing formalist conception makes their theory crucially different from Cantor's, Frege argues; and in § 131 he speaks of 'the incorrigible disparity between what the introduction of irrationals demands and what formalist arithmetic can offer', due to the fact that 'to

introduce irrationals, we need infinitely many numbers', whereas 'formalist arithmetic has only a finite set of numerical figures'. The claim of radical formalism is to dispense with the need to vindicate the possession either of meaning or of truth by mathematical statements, or the possession of reference by mathematical terms. Thus in § 87 Frege quotes Heine as saying, 'I call certain tangible signs numbers: the existence of these numbers is thus not in question'. It is then impossible, Frege argues, for the formalist to explain what he understands by an infinite sequence of numbers without abandoning his claim to this alleged advantage. Frege derives much amusement from the formalists' contortions as they attempt to extricate themselves from this dilemma.

More sophisticated formalists than those with whom Frege had to contend would admit that they were concerned with abstract symbol-types rather than physical tokens, and would avoid talking about infinite sequences of terms for rational numbers. Instead, they would consider what theory would be developed by one who believed in rational numbers distinct from but denoted by such terms, and in infinite sequences of them. They would next think how to axiomatise that theory, and then how to formalise it. At that point, they would throw away the meaning that had guided them to the formal theory, and declare mathematicians to be concerned solely with the production of formal proofs within that formal theory. Frege cannot be blamed for the naivety of the formalism of his day; but his third objection could not be raised against a more sophisticated version of it. We learn from it little about his own conception of the system of real numbers, save that it is not merely an infinite totality, but contains elements that are themselves infinite in character, in the sense that they are not in general capable of being specified by a finite description.

Theory and metatheory

Frege's second objection, interspersed with other matter relating to the specific formulations of Heine and Thomae, is expounded at great length, and occupies §§ 93–119. Nothing in the argument bears particularly upon the nature of real numbers. It is an objection to global formalism, not to a version directed only at a particular mathematical theory: and it is Frege's principal ground for rejecting it.

In § 88 he quotes Thomae as comparing arithmetic, as the formalist conceives it, with the game of chess; and in § 93 he draws the contrast, which, as he remarks, Thomae fails to do, between the game and its theory. Neither chess moves nor positions on the chessboard express anything; in particular, they do not express thoughts that can be evaluated as true or false. Given the game of chess, however, nothing can stand in the way of our developing the

theory of chess: this consists of meaningful propositions about chess moves and chess positions, capable of demonstration in the same way as mathematical theorems. In the same way, given an uninterpreted formal theory, nothing can stand in the way of our constructing its metatheory, whose subject-matter will be the formulae and formal proofs of the uninterpreted theory. The propositions of the metatheory will, again, be meaningful statements, established by deductive proof. The formalist has no way of preventing this metatheory from arising: if we have any reason to be interested in the formal theory, we shall be interested in metatheoretical results concerning it. The formalist can allow no place for the metatheory in his philosophy of mathematics, however. It is not a mathematical theory, on his understanding of what a mathematical theory is, for it has content: but it is not an empirical theory, either, and still less is it an *application* of the formal theory.

The formalist cannot block the development of a mathematical theory: he can only formalise it. Frege did not have to respond to a formalist answer to his objection about the theory of chess, that it, too, could be formalised, as could the metatheory of a formalised mathematical theory. If he had, it would have been open to him to reply that formalisation of the theory of chess would in no way prevent anyone from asserting the meaningful propositions of the intuitive theory: he wants, not to derive a formula in a formal theory, but to assert, for example, that it is impossible to force mate with a king and two knights. The formalist must consider such a proposition as an *application* of the formalised theory of chess – an application of a formal theory made before that formal theory existed. (This links Frege's first objection to formalism with his second, that it can give no good account of the application of mathematics.) In the same way, the proposition that (formalised) Peano arithmetic is consistent (Gentzen), or that, if it is consistent, there is an undecidable sentence (Gödel), is to be regarded as an application of the formalised metatheory. (We should not take even the second of these propositions as an application of the formal system itself, since we do not have a derivation in that system of its arithmetised version, but only a proof in the metatheory that it is derivable.)

The formalist is not merely engaged in drawing the boundary between pure and applied mathematics in an unusual place: he is creating a hitherto unrecognised region. For the intuitive theory of chess differs from applications of mathematics in the physical sciences in being itself *a priori*. Applications of mathematics in physics depend on facts established by empirical observation, or on theoretical hypotheses: but the theory of chess depends only on such premisses as that the king must move out of check, which is not a theoretical hypothesis but a rule constitutive of the game of chess, just as the axioms and rules of Peano arithmetic are constitutive of that formal system. The formalist is thus compelled to recognise a realm of meaningful *a priori* theories, to which he denies the title of pure mathematics, but which contain theorems and proofs of theorems, and would be regarded by everyone else as clear

examples of mathematical theories. He has not succeeded in abolishing meaningful mathematics: he has merely relabelled it.

To be more precise, he has merely restricted the subject-matter of what he is prepared to recognise as pure mathematics to the study of formal systems: all pure mathematics, for him, is metamathematics. The interest of a derivation in a formal system lies in its being a demonstration that the formula derived can be derived in that system. The formalist has not, therefore, expelled meaning from mathematics: he has merely shifted the mathematical proposition from the content of the formula to the metatheoretical statement that it is derivable. For the interest of such a statement is continuous with that of other metatheoretical statements not demonstrable by means of a derivation in the formal system, such as that a certain formula is not derivable, or that neither it nor its (formal) negation is derivable, or that every formula of a certain form is decidable. This is because the statement that a given formula is derivable is still a meaningful *a priori* statement in a language. As such, nothing can inhibit us from applying to it the usual logical operations of negation, generalisation and the rest, or, indeed, from generalising at a yet higher level to such propositions as that every consistent formal system of a certain class is incomplete. There is no gulf within mathematics comparable to that between a position on the chessboard and the proposition that mate cannot be forced with two knights: the principle of duality is a theorem of projective geometry, and does not differ from, say, Desargues's theorem as something of an utterly different character.

Global formalism, in its radical version, promised to clear up all the problems of the philosophy of mathematics by ruling them out of order: once meaning had been expelled from mathematics, those problems could simply no longer arise. Once formalism had shed its crudest formulations, it could not continue to maintain, as Heine attempted to maintain, that mathematics is solely concerned with actual marks made with ink or chalk; it had to allow its subject-matter to consist of strings of symbols considered as types, and thus of objects as abstract as the natural numbers. Reflection on Frege's crucial distinction between chess and the theory of chess, and between a formal theory and its metatheory, rapidly dispels the remaining claim of radical formalism, that mathematicians are not concerned to make meaningful assertions, and *a fortiori* not to make meaningful assertions *a priori*: with that, global formalism evaporates as a tenable interpretation of mathematics.

Application

Frege's first objection to radical formalism is expounded in §§ 89–92. These paragraphs are of high importance for the understanding of Frege's theory of real numbers, which he evidently had vividly in mind while writing them. They are also of high importance for a topic central to his philosophy of mathematics

generally, since they form the only sustained discussion of the application of mathematics in the whole of his writings. Scattered remarks on the subject, such as those to be found in *Grundlagen*, § 9, can easily give the utterly false impression that he regarded mathematics as concerned with an ideal realm of pure abstract objects having far less bearing upon empirical reality than the galaxies have on terrestrial affairs.

The formalist expressly views a mathematical theory as a type of game. Frege insists that it is not a game, but a science in the general sense of a sector in the quest for truth. What makes it a science, he claims, is precisely that it is capable of being applied: 'it is applicability alone', he declares at the end of § 91, 'that raises arithmetic from a game to the rank of a science. Applicability therefore belongs necessarily to it.'

The formalist, according to Frege, cannot explain, or even recognise, the applicability of a mathematical theory. He cannot do so because, for him, the formulas of the theory express no thoughts: they are not meaningful statements, to be judged true or false, but mere formal objects. 'Why can one not make an application of a chess position?', Frege asks, and answers, 'Obviously because it does not express a thought'. 'Why can one make applications of arithmetical equations?', he continues, and replies, 'Only because they express thoughts'. The formalist chooses to regard the equations as stripped of their content, and hence as of the same nature as positions on the chessboard. He thereby 'excludes from arithmetic that which alone makes it a science'.

Why does Frege think it necessary, for a mathematical formula to be applied, that it express a thought? Plainly because he takes the application of a mathematical theorem to be an instance of deductive inference. It is possible to make an inference only from a thought (only from a true thought, that is, from a fact, according to Frege): it would be senseless to speak of inferring to the truth of some conclusion from something that neither was a thought nor expressed one. We do not, of course, call every inference an 'application' of its premisses: it is in place to speak of application only when the premisses are of much greater generality than the conclusion.

Frege tacitly took the application of a theorem of arithmetic to consist in the instantiation, by specific concepts and relations, of a highly general truth of logic, involving quantification of second or yet higher order: if the specific concepts and relations were mathematical ones, we should have an application within mathematics; if they were empirical ones, we should have an external application. Mathematical theories could not themselves consist solely of logical truths involving only higher-order quantification, since they required reference to mathematical objects (which Frege believed he could analyse as logical objects), above all in order to maintain the extensional distinctness of the concepts and relations quantified over, which might collapse on one another if the domain of the individual variables were allowed to be too sparse. When we are concerned with applications, however, the objects of the mathematical

theory play a lesser role, or none at all, since we shall now be concerned with the objects of the theory to which the application is being made: application can therefore be regarded as consisting primarily of the instantiation of highly general truths of logic. Evidently, a formalist can allow no place for application as so conceived.

The formalist could object that he is not bound by Frege's conception of application: he can propose an alternative conception, according to which application consists in supplying a specific interpretation for an intrinsically uninterpreted formal system. In practice, such interpretations will display a common pattern. Although, officially, the symbols of the formal system are all of them unmeaning marks, they will unofficially be subject to a syntactic classification: logical constants will be recognised as such, and, usually, as bearing the specific meanings of the operators of classical logic; individual constants, predicate letters, function symbols and so on will all be viewed as belonging to their respective syntactic categories. In short, the notion of interpretation employed will be that used in standard model theory, rather than that appropriate for someone trying to break an intricate code or decipher an unknown script; if there proved to be a successful interpretation of this latter sort that did not respect the apparent syntax of the formal language, it would not be intuitively regarded as an application of the mathematical theory, but merely as an astounding coincidence. Furthermore, the pattern common to the various interpretations would be likely to be discernible in much more than the syntactic structure of the formal language: when made explicit, it would closely resemble the meaningful version of the theory as Frege conceived of it. It demands proof that the formalist has at his command a conception of application genuinely distinct from that of Frege; and the proof seems unlikely to be forthcoming.

In any case, the formalist cannot, consistently with his position, regard the applications of a mathematical theory as any part of the business of mathematics. For Frege, it is here that the gravest error of formalism lies; but his position is a subtle one. He would agree that specific applications of arithmetic are *not* the business of arithmetic, but only of the particular science within which those applications are made. That is the reason for his rebuke to Mill, in *Grundlagen*, § 9, for 'confusing the applications that one can make of an arithmetical proposition . . . with the pure mathematical proposition itself'. It is this remark, above all, that may mislead the reader into supposing Frege to have believed arithmetical propositions to have senses entirely unconnected with their applications and to have regarded those applications as wholly external to mathematics and of no concern to it. His discussion of the application of mathematics in *Grundgesetze*, Volume II, §§ 89–92, by contrast, reveals an attitude diametrically opposite to this. Had he altered his view between writing *Grundlagen* and writing the second volume of *Grundgesetze*? By no means. In a footnote attached to § 137, at the very end of the section on the

formalists, and devoted to Helmholtz,¹ another exponent of an empiricist philosophy of mathematics, Frege accuses Helmholtz of making the very same mistake as that of which, in *Grundlagen*, he had accused Mill. The mistake is that of 'confusing the applications of arithmetical propositions with the propositions themselves, as if the question concerning the truth of a thought and that concerning its applicability were not quite distinct'; Frege objects that 'I can very well recognise the truth of a proposition without knowing whether I shall be able to make any application of it'.

It may seem that, when Frege is criticising formalism, he treats the applications of a mathematical theory as intrinsic to it, but, when he is criticising empiricism, he treats them as extrinsic; but, surely, he could not be so careless as to juxtapose expressions of these incompatible views. In fact, his views are not incompatible. What are extrinsic to arithmetic are all *particular* applications of it: these relate to restricted domains of knowledge, and, as Frege says in *Grundlagen*, 'often . . . presuppose observed facts'. The mistake of Mill and of Helmholtz consists in taking such particular applications as integral to the senses of arithmetical propositions. What is intrinsic to arithmetic, by contrast, is the general principle that explains its applicability and hence determines the common pattern of all particular applications. The mistake of the formalists consists in ignoring this, or, at best, reckoning it not to be the business of arithmetic.

In §§ 87 and 88, Frege alludes to his conception of the real numbers, as being ratios between quantities, which he has already introduced in the preceding section (*b*) on Cantor. In § 92 he makes telling use of it in explaining his ideas about the relation of arithmetic to its applications. 'We know,' he says, 'that the same ratio between quantities (the same number) can occur in connection with lengths, with temporal durations, with masses, with moments of inertia, etc. This makes it probable that the problem how we are able to make use of arithmetic is to be solved, at least in part, independently of those sciences within which the application is made.' Frege is here asserting that the applications of the theory of real numbers, though various, are not simply heterogeneous. On the contrary, they display a common pattern. Arithmetic should not concern itself with particular applications, even when they do not depend upon contingent laws, since they involve concepts alien to it, like length or mass, which belong to geometry, physics or some other special domain of knowledge. It ought, on the other hand, to concern itself with the *general* notion of quantity, because this concept has the right degree of generality, and underlies all particular uses of the real numbers as measures.

It is its capacity to be applied that raises arithmetic to the rank of a science. The formalist regards this as irrelevant to arithmetic itself: 'is it well done',

¹ Specifically, his essay 'Zählen und Messen erkenntnistheoretisch betrachtet', about which Frege says, 'I have scarcely ever encountered anything so unphilosophical as this philosophical essay'.

Frege asks at the end of § 91, 'to exclude from arithmetic what alone makes it a science?' But the applicability of arithmetic sets us a problem that we need to solve: what makes its applications possible, and how are they to be justified? We might seek to solve this problem piecemeal, in connection with each particular application in turn. Such an attempt will miss its mark, because what explains the applicability of arithmetic is a common pattern underlying all its applications. Because of its generality, the solution of the problem is therefore the proper task of arithmetic itself: it is this task which the formalist, who regards each application as achieved by devising a new interpretation of the uninterpreted formal system and as extrinsic to the manipulation of that system, repudiates as no part of the duty of arithmetic. 'But what is then really achieved by this?', Frege asks at the beginning of § 92, and answers:

Admittedly arithmetic is relieved of some work; but is the task thereby removed from the world? The formalist arithmetician seeks to shift it on to the shoulders of his colleagues the geometer, the physicist and the astronomer; but these decline with thanks to occupy themselves with it: and so it falls between these sciences into the void. A clean demarcation between the domains of knowledge may be good; but it ought not to be carried out in such a way that one domain remains over, for which no one will undertake the responsibility.

On the contrary, Frege concludes,

it is reasonable to demand this work of the arithmetician, in so far as he can accomplish it without encroaching on those special domains of knowledge. For this he needs, above all, to attach a sense to his formulas; and this will then be of so general a kind that, with the help of geometrical axioms and of physical and astronomical observations and hypotheses, it can find manifold applications within these sciences.

So far from having accorded scant respect to the applications of mathematics, Frege was, of all philosophers of mathematics, the one who gave the greatest attention to the topic.

Waismann's critique of Frege

With Wittgenstein, Friedrich Waismann, writing from a Wittgensteinian standpoint, was one of the few to comment on Frege's critique of formalism. In § 91, Frege wrote:

Why can no application be made of a chess position? Obviously, because it expresses no thought. If it did so, and every chess move conforming to the rules corresponded to a transition from one thought to another, applications of chess would also be conceivable.

In the course of a long discussion of Frege's argument, Waismann commented as follows:²

What, then, has to be added, in order for a mathematical equation to express a thought-content? Application, and nothing more. It is mathematics when the equation is used for the transition from one proposition to another; otherwise it is a game. To say that a move in chess expresses no thought is hasty; for it wholly depends on us.

Waismann goes on to imagine positions of chessmen being used to represent the disposition of troops in a battle, and continues:

'Because a chess move expresses no thought, one cannot apply it.' Would it not be more correct to say that because we have not provided an application for it, the chess move does not express a thought?

Waismann was not a formalist, and did not deny that a mathematical proposition has a sense. Yet the position he here adopts is barely distinguishable from that of the radical formalist who construes application as imposing an empirical interpretation upon a hitherto uninterpreted formal calculus. Waismann denies that we first confer a sense on the proposition, and then, in the light of that sense, make various applications of it: rather, we make the applications, and *thereby* give it a sense – a truly Wittgensteinian idea. What is so applied must therefore be an uninterpreted formula, just as the formalist supposes: the only difference between him and Waismann is that he expressly denies that the application has anything to do with mathematics.

It is difficult to find a psychologically convincing example of what Waismann has in mind; but we might imagine a child who has in school been taught computations with fractions in a purely formal manner. Waismann and Frege would agree that he does not yet attach any sense to the equations: they merely figure in a calculating game. He is then for the first time shown how to measure lengths with a ruler, and how to construct rulers divided into tenths, twelfths and ninths of an inch. Certainly he now attaches a certain sense to the equations: but is his experience a refutation of Frege's view? Plainly not. The temporal order of his attaching that sense to them and his mastering the application is not to the point: what matters is that, although he has now begun to understand equations involving fractions, he does not yet fully understand them. He does not do so because the sense he has learned to attach to such equations lacks the generality required of the full arithmetical sense: it is not an arithmetical sense, but a geometrical one. It relates to just one kind of application; the child has yet to acquire a grasp of the general use of rational numbers to give the magnitude of quantities of different kinds. Like the

² F. Waismann, *Einführung in das mathematische Denken*, second edn., Vienna, 1936, p. 165; English translation by T.J. Benac, *Introduction to Mathematical Thinking*, New York, 1951, p. 240.

formalists' view, Waismann's allows no place for this: just that is what is wrong with it from Frege's standpoint. Waismann believed that he had seen much further than Frege; but he had not seen as far.

Retrospect and prospect

After the critical sections, Frege concludes Part III.1 with two brief further sections. Section (*f*), entitled 'Retrospect and prospect', and running from § 156 to § 159, aims to draw the moral of the long examination of other theories of real numbers that has preceded it; section (*g*), entitled 'Quantities', and running from § 160 to § 164, gives a preliminary sketch of the theory of real numbers to be developed formally in Part III.2. Section (*f*) contains few surprises, and may be reviewed at this point.

Frege begins in § 156 by recalling the gross methodological errors: formalism; the mistake of thinking that merely defining a concept guarantees the existence of an object falling under it; and the belief that its existence will be secured provided that the concept can be shown to be consistent. Its inconsistency cannot be relied on to be obvious, however, and so its consistency needs to be proved. The only known way of proving it is by finding an object that falls under the concept; 'until a quite new principle for proving freedom from contradiction is discovered, we can advance no further along this route'.

In § 157, Frege reiterates that he can, by means of value-ranges, achieve what other mathematicians hope to gain by creative definitions.³ Furthermore, he has already fixed on construing real numbers as ratios of quantities, and hence on quantities as being the objects between which such ratios obtain; he remarks in a footnote on his agreement with Newton in this respect.⁴ There was quoted in the last chapter the passage from § 157 in which Frege distinguishes cardinal numbers (*Anzahlen*), as answering the question, 'How many objects of a certain kind are there?', from real numbers, as used to say how great a given quantity is compared to a unit quantity, and concludes that the two kinds of number form disjoint domains.

In § 158 Frege warns against making essential appeal to geometry. 'If arithmetical propositions *can* be proved independently of geometrical axioms, then they *must* be. To do otherwise would be needlessly to belie the autonomy of arithmetic and its logical nature.' Writers on geometry sometimes begin by speaking of the line segment *a* and later use '*a*' to denote the number giving the length of the segment; this results in the confused idea that a numerical symbol does or can refer to a line segment. It refers, rather, to a ratio of quantities, here of the length of the segment to that of a unit segment: a ratio that can also obtain between masses, between temporal durations, etc. 'The

³ In other words, logical abstraction is to be used in defining the real numbers, as foretold in *Grundlagen*, § 104.

⁴ See *Grundlagen*, § 19.

real number is thereby detached from all particular types of quantity, and, at the same time, governs them all.'

In § 159, Frege recognises his approach as for this reason being intermediate between the old method of founding the theory of irrational numbers on geometry, and the type of purely arithmetical method introduced 'in more recent times'.⁵ Characterising this intermediate approach he says:

From the former we retain the interpretation of a real number as a ratio of quantities or measurement-number; but we detach it from geometrical quantities, and from all particular types of quantity, and thereby come closer to the more recent attempts. But at the same time we avoid the defect apparent in the latter: that either measurement does not appear at all; or else it is tacked on in a purely external fashion, without any inner connection founded upon the essence of number. It follows from the latter approach that it has to be separately stated for each type of quantity what it is to measure it and how one may thereby obtain a number. A general criterion is then completely lacking for when the numbers can be used as measurement-numbers and for the form that this application of them will then take.

This passage contains the only explicit formulation in Frege's writings of a methodological maxim implicit in his practice. It is not enough that an arithmetical theory should undertake to state and justify the general principles governing its application. It is necessary, further, that they should not be 'tacked on' as an appendage to the theory, as Dedekind did with his foundations for number theory and Cantor did with his method of introducing the real numbers; rather, they belong to the essence of number, and hence should be made central to the way the numbers are defined or introduced.

We may thus hope, Frege concludes, neither to relegate the treatment of the application of real numbers to the various special domains of knowledge nor to sully arithmetic with objects, concepts and relations borrowed from those sciences, thus endangering its autonomy and its essential character. The particular applications are indeed not the concern of arithmetic; but a treatment of the general principle underlying all applications may legitimately be demanded of it.

Frege closes section (*f*) with a problem about the execution of his programme; how, 'if reference to geometrical and physical quantities is forbidden', can we find quantities that stand, one to the other. in the ratio $\sqrt{2}$, which we must do if the existence of that irrational number is to be proved? He postpones suggesting any solution to this problem until § 164.

⁵ Frege cites Hankel's *Theorie der complexen Zahlensysteme* of 1867 as an example of the former; by the latter he means the theories of Cantor and Dedekind, introduced simultaneously in 1872, though he does not mention them by name.

CHAPTER 21

The Critique of Cantor

Section (b) of *Grundgesetze*, Part III.1, occupying §§ 68–85, is devoted to a critique of Cantor's method of introducing irrational numbers, which was a modification of that of Weierstrass.¹ In it, Frege goes to the heart of his dissatisfaction with existing definitions of the real numbers; we learn much from this section of his ground for adopting his own divergent approach. The discussion is nevertheless not clean: it does not confine itself to matters of fundamental principle, but includes many objections to mistakes on Cantor's part due solely to carelessness and easily remedied.

Fundamental series

Cantor first expounded his theory of real numbers in 1872.² He did so afresh in § 9 of his *Grundlagen* of 1883.³ Frege principally examines the exposition in Cantor's *Grundlagen*, but, in §§ 75 and 85, quotes also from the article of 1872.

The main course of Frege's argument begins in the first two paragraphs, §§ 68–9, and then breaks off for a partial digression running from § 70 to § 76; the main argument then resumes at § 77 and continues until the end of the section in § 85, although the main line and the digression cannot be quite cleanly disentangled.

¹ This section is considered in this chapter in greater detail than others, in part because of its importance, and also because, together with sections (e), (f) and (g), it is not available in English translation.

² 'Über die Ausdehnung eines Satzes aus der Theorie der trigonometrischen Reihen', *Mathematische Annalen*, vol. V, 1872, pp. 123–32, the relevant sections being §§ 1 and 2, pp. 123–8; reprinted in G. Cantor, *Gesammelte Abhandlungen mathematischen und philosophischen Inhalts*, ed. E. Zermelo, Berlin, 1932, reprinted 1980, pp. 92–102 (§§ 1–2 are on pp. 92–7).

³ G. Cantor, *Grundlagen einer allgemeinen Mannichfaltigkeitslehre*, Leipzig, 1883. This was a reprint of the fifth article in his series 'Über unendliche, lineare Punktmannichfaltigkeiten', *Mathematische Annalen*, vol. XXI, 1883, pp. 545–91, and reprinted in turn in his *Gesammelte Abhandlungen*, pp. 165–208. It will be cited here throughout as *Grundlagen*; the page numbers will be those of the separate edition, with those of *Mathematische Annalen*, followed by those of *Gesammelte Abhandlungen*, in brackets.

Cantor defines a *fundamental series* to be a sequence of rationals satisfying the Cauchy condition for convergence.⁴ Frege begins by citing this definition, together with Cantor's additional remark, 'I call [this] a fundamental series and associate to it the number b to be defined'.⁵ Frege makes play with the question whether, in this sentence, Cantor intended by 'number' a numerical symbol or the referent of such a symbol. He considers first the former interpretation, which he understands as meaning that the symbol is to denote the series itself; as he remarks, under this interpretation nothing essential is accomplished by selecting a particular symbol to denote the series. He goes on to quote Cantor as distinguishing three cases:

- (1) for any given positive rational ϱ , there is a term of the series such that the absolute value of every subsequent term is less than ϱ ;
- (2) for some positive rational ϱ , there is a term of the series such that every subsequent term is greater than ϱ ;
- (3) for some negative rational $-\varrho$, there is a term of the series such that every subsequent term is less than $-\varrho$.

'In the first case', Frege quotes Cantor as saying, 'I say that b is equal to nought, in the second case that b is greater than nought or positive, and in the third case that b is smaller than nought or negative.' Frege justly complains of these definitions on the ground, first, that, in each, two expressions ('nought' and 'equal to', 'greater than' or 'less than') are being defined simultaneously, and, secondly, that in any event these expressions must be taken as already known and hence not open to further definition. He fails to point out the easy remedy, namely to delete the words 'greater than nought or' and 'smaller than nought or' in the second and third, and, in the first, to substitute for 'is equal to nought' a simple predicate such as 'vanishes'.

In § 70 Frege cites an article by Eberhard Illigens criticising Cantor's theory;⁶ it is this that leads to the digression. As Frege remarks, Illigens adopts the same interpretation of the phrase 'I associate to it the number b ' as he is currently assuming, taking ' b ' to be a symbol denoting the fundamental series. Frege reports Illigens as objecting that a series of numbers cannot itself be a quantity, and hence that the terms 'greater' and 'smaller', as applied to them, cannot have a sense analogous to that in which they express relations between rational numbers. Frege's comment is that Cantor does not have to be under-

⁴ It is convenient to follow Cantor's terminology and speak of a 'series' where we should now say 'sequence'.

⁵ G. Cantor, *Grundlagen*, p. 23 (567, 186).

⁶ E. Illigens, 'Zur Weierstrass'-Cantor'schen Theorie der Irrationalzahlen', *Mathematische Annalen*, vol. XXXIII, 1889, pp. 155-60.

stood in the way Illigens understands him, namely as confusing the sign with what it signifies, but that there is nevertheless something correct in Illigens's objection. In § 76 we learn that this consists essentially in the absence from Cantor's theory of the principal thing, the real numbers themselves, at least on the interpretation of Cantor's words that Frege is currently assuming. Since, on that interpretation, the symbols such as ' b ' are dispensable, we have only the fundamental series, and no real numbers. A fundamental series might serve to determine a particular real number, if we knew what real numbers were; but just this is what we have not been told.

This conclusion leads Frege, in § 77, to doubt whether the interpretation of Cantor's phrase 'I associate to [the fundamental series] the number b ' which he has hitherto been assuming can accord with Cantor's true intention. He therefore now conjectures instead that Cantor intended, not to select a mere label for each fundamental series, but to associate with it a number; such a number would not, in general, be a rational. 'These numbers', Frege comments, 'are therefore in part new ones, that have not as yet been considered, and they are to be determined precisely by the fundamental series with which they are connected.' This emended interpretation is so obviously correct that the reader may feel some impatience at the time wasted by scrutinising Cantor's theory in the light of the old one; but, from Frege's standpoint, the new interpretation hardly improves matters. The burden of Frege's complaint against Cantor, so interpreted, is that at no point does he provide any account of how the new numbers are to be defined, nor of how, when they have been defined, their association with the fundamental series is to be specified.

In §§ 79–81 Frege quotes Cantor's immediately following remarks:⁷

Now come the elementary operations. If (a_ν) and (a'_ν) are two *fundamental series* by which the numbers b and b' are determined, it is demonstrable that $(a_\nu \pm a'_\nu)$ and $(a_\nu \cdot a'_\nu)$ are also *fundamental series*, which therefore determine three new numbers that serve me as definitions of the sum and difference $b \pm b'$ and of the product $b \cdot b'$.

It is slightly odd that Frege should here omit Cantor's corresponding definition of division; for, while excluding division by the real number 0, or by a fundamental series that vanishes (converges as a sequence to 0) – thus offending against Frege's principle that a function must be everywhere defined – Cantor neglects to provide for the case when a *term* of the fundamental series by which the divisor is given is 0. Here, then, Frege had an unquestionable mistake to complain of, although, again, one easily rectified; he surprisingly overlooks the opportunity.

Cantor's next two sentences are once more quoted by Frege in full:

The elementary operations upon a number b given by a fundamental series (a_ν)

⁷ G. Cantor, *Grundlagen*, pp. 23–4 (568, 186–7).

and a directly given rational number a are included in the above stipulations by letting $a'_\nu = a$, $b' = a$.

Only now come the definitions of being equal to, greater than and smaller than, as between two numbers b and b' (of which b' may = a): one says, namely, that $b = b'$ or $b > b'$ or $b < b'$ according as $b - b'$ is equal to nought or greater or less than nought.

Frege interprets these stipulations as accomplishing no more than to supply hints concerning which numbers are to be associated with particular fundamental series: in no case do they specify the association uniquely, nor, more importantly, do they supply us with any new numbers to associate with those fundamental series that do not converge to rational limits.

Frege subsequently half-admits that, in making the first of these two objections, he has gone too far. He is interpreting Cantor, apparently correctly, as *adding* irrational numbers to the already given system of rational numbers: some of the numbers to be associated with fundamental series will be already known ones, namely rationals, and others will be new ones, the newly introduced irrationals. In § 78 Frege allows that the stipulation that the number associated with a fundamental series whose limit is 0 shall be 'equal to nought' must mean that the (rational) number 0 shall be associated with every such fundamental series, at least if we understand Cantor as meaning 'identical' by 'equal'. In § 81 Frege recognises it as Cantor's intention that the same number shall be associated with two fundamental series (a_ν) and (a'_ν) if the fundamental series $(a_\nu - a'_\nu)$ has the limit 0. Since it is also plainly Cantor's intention that the rational number a shall be associated with the fundamental series (a'_ν) every term a'_ν of which is a , the association of rational numbers with fundamental series has been fully provided for.

Frege insists, however, on his second objection. Cantor's stipulations achieve, at best, only an association of *rational* numbers to certain fundamental series: it does not yield any definition of irrational numbers. Furthermore, the concession depends upon assuming that, as Cantor uses it, 'equal' means 'identical': and Frege professes to be doubtful whether it *can* mean that, for, if it did, it would not be open to be defined. But, if it does *not* mean 'identical', then the stipulations do not even determine that the number 1 is to be associated with the fundamental series (a_ν) for which $a_\nu = \nu/(\nu + 1)$ for every ν ; or so Frege claims in § 84.

In general, Frege says in § 83, 'the words "equal", "greater", etc., are left in a perpetual oscillation between being known and being unknown'; thus, he charges in § 81, 'at one moment the words "equal", "greater", "smaller", "sum" and "product" appear as known, immediately thereafter as unknown and then again as known'. When Cantor defines these words as applying to the new numbers he is purporting to introduce, it is because we cannot help adverting to their usual meanings that we wrongly suppose such a symbol as Cantor's ' b ' to have some specific content, Frege argues in § 82. He proceeds

in § 83 to play the game he also practised on Hilbert, using nonsense words in place of 'equal', 'nought' and so on in order to show that Cantor has not properly succeeded in defining anything.

The foregoing criticisms of Cantor by Frege are perfectly sound, but intolerably laboured. As with Dedekind, Russell made very similar criticisms, going so far as to say outright that 'there is absolutely nothing in the above definition of the real numbers to show that a is the real number defined by the fundamental series whose terms are all equal to a . The only reason why this seems self-evident is that the definition by limits is unconsciously present, making us think that, since a is plainly the limit of a series whose terms are all equal to a , therefore a must be the real number defined by such a series.'⁸ Russell's point is that, contrary to Cantor's intentions as interpreted by Frege, the rationals and the real numbers form disjoint domains: 'a fundamental series of rationals *defines* a real number, which is never identical with any rational.'

We cannot but deprecate the disagreeable tone of Frege's criticisms of Cantor, although, in view of Cantor's mean-spirited review of his *Grundlagen der Arithmetik*,⁹ we may forgive it. In view of the fact that, as we shall see, he did, in part of his section (b) on Cantor, go to the heart of his disagreement with him, we may also forgive the heavy-handed style of the peripheral criticisms (which no doubt did not seem peripheral to Frege) just reviewed. It is obvious to us how Cantor might have emended his theory so as to escape Frege's objections. Having distinguished his three classes of fundamental series, as vanishing, positive and negative, he should, without yet speaking of numbers correlated to fundamental series, have defined the difference operation upon them, proving that $(b_v - b'_v)$ is a fundamental series if (b_v) and (b'_v) are. This would have enabled him to define (b_v) to be *equivalent* to (b'_v) just in case $(b_v - b'_v)$ vanishes. The next step would then have been to show this relation of equivalence to be transitive and hence a genuine equivalence relation. The way would then have been open to define the real numbers by logical abstraction, that is, as equivalence classes of fundamental series; the 'elementary operations' on real numbers could then have been defined in terms of the corresponding operations on fundamental series, after showing equivalence to be a congruence relation with respect to the latter operations.

Faced with a theory presented as Cantor presented his, we automatically transform it in such a manner as that just suggested. This is because rigour of presentation is second nature to modern mathematicians. It was not second nature to Cantor, as his simultaneous introduction of the three elementary operations, on fundamental series and on the numbers correlated with them, abundantly demonstrates; nor was it second nature to most of the mathema-

⁸ B. Russell, *Principles of Mathematics*, § 269.

⁹ *Deutsche Literaturzeitung*, vol. 6, 1885, col. 728-9, reprinted in G. Cantor, *Gesammelte Abhandlungen*, pp. 440-1. In his own *Grundlagen*, §§ 85-6, Frege had written in terms of high respect and appreciation of Cantor's *Grundlagen* of 1883.

ticians of the time. Frege had won through to what we think of as a modern standard of rigour (if we trouble to think of it at all) by hard reflection on legitimate methods of defining mathematical notions: we can therefore hardly blame him for his increasingly ponderous insistence on it, even though we have little to learn from it. What we can complain about is his failure to indicate how Cantor's theory could have been emended, which to him, the great exponent of definition by logical abstraction, must have been as obvious as it is to us.

Fortunately, criticisms of this nature do not exhaust Frege's observations on Cantor's theory of real numbers; in part of section (b) he explained the deep grounds for his rejection of it. Together with the remarks about application in the subsequent section on the formalists, this constitutes the essential preliminary to Frege's own theory of real numbers.

Frege's digression

In the digression running from § 70 to § 76, the trend of Frege's remarks is not explicitly distinguished from his general contention, that Cantor's construction of the real numbers is logically faulty. In the course of the digression, he nevertheless offers reasons why he would not have found Cantor's theory acceptable even if it had been emended in the way suggested; it is for this reason much the most interesting part of his critique of Cantor.

Frege's starting-point in § 70 is the remark he cites from the article on Cantor by Illigens that the Cantorian symbols such as '*b*', which he takes to denote fundamental series, cannot denote quantities (*Quantitäten*), as the rational numbers do; he quotes Illigens as saying, 'The symbols for series of numbers lack the capacity to become concepts of quantity, in spite of the labels attached to them by the various definitions', and, in § 71, as concluding that Cantor's theory does not enable us to say what a line $\sqrt{2}$ metres long may be. As Frege observes, if this, as stated, were a valid objection, it would convict Cantor of a confusion between sign and thing signified, or, as we should say, between use and mention; but he defends Cantor against the charge, remarking, however, that 'there is nevertheless something true in this objection'. Frege's difficulty, as he clearly perceives, is that it is Illigens himself who constantly confuses sign and thing signified: he has, therefore, to tread carefully in offering him partial support.

Illigens's observations prompt Frege to comment on the use of numbers as measures of quantity. He first rebukes Illigens for speaking of rational numbers as symbols for quantities. 'According to linguistic usage one calls lengths, surface areas, angles, temporal intervals, masses and forces "quantities". Is it then correct to say that either the number $2/3$ or the numerical symbol " $2/3$ " denotes a certain length, or a certain angle, or, indeed, both?', he asks in § 71. In § 72 he quotes A. Pringsheim as explaining that the rational numbers are

signs that *can* represent definite quantities, but *need* not do so,¹⁰ and has little trouble ridiculing this remark.

'How, then, does it really stand with the assertion that numerical symbols designate quantities?', Frege asks in § 73. His answer brings him closer to the heart of his own views concerning real numbers than anything else in the critical sections of Part III.1 of *Grundgesetze*, and is worth quoting in full.

Let us look at the applications of arithmetical laws in geometry, astronomy and physics. Here numbers in fact occur in connection with quantities (*Größen*)¹¹ such as lengths, masses, intensities of illumination and electrical charges; and, upon superficial consideration, one might think that the same numerical symbol referred now to a length, now to a mass, now to an intensity of illumination. This would then appear to support Pringsheim's assertion that between the numerical symbols and the quantities there exists a certain connection, but only a loose one. Let us examine this more carefully. What is it that we really apply, when we make use of an arithmetical statement? The sound of the words? Groups of special figures, consisting of printer's ink? Or do we apply a thought-content that we connect with those words or with those symbols? What do we prove, when we prove an arithmetical statement? That sound? Those figures? Or that thought-content? Why, of course, this last. Very well, then: we must have a particular thought as the content of the statement, and this we should not have, if the numerical symbols and number-words occurring in it referred, now to this, now to that.

If we look more carefully, we notice that a numerical symbol cannot by itself on its own denote a length, a force or the like, but only in combination with an expression designating a measure, a unit, such as a metre, a gram, etc. What, then, does the numerical symbol on its own refer to? Obviously a ratio of quantities.¹² This fact lies so close to hand that it is not surprising that it has long been recognised. If, now, we understand by 'number' the referent of a numerical symbol, a *real number* is the same as a ratio of quantities. Now what have we gained by defining *real number* as 'ratio of quantities'? At first it seems merely that one expression has been replaced by another. And yet a step forward has been taken. For, first, no one will confuse a ratio of quantities with a written or printed symbol; and so one source of countless misunderstandings and errors is blocked. Secondly, the expression 'ratio of quantities' or 'ratio of one quantity to another quantity' serves to indicate the manner in which real numbers are connected with quantities. Admittedly, the principal work remains to be done. We have as yet no more than words which indicate to us only approximately the direction in which the solution is to be sought. The reference of these words has yet to be more precisely fixed. But we shall now no longer say that a number or numerical symbol denotes, now a length, now a mass, now an intensity of illumination. We shall say, rather, that a length can have to another length the same ratio as a mass has to another mass, or as an intensity of illumination has to an intensity of illumi-

¹⁰ A. Pringsheim, article in the *Encyklopädie der mathematischen Wissenschaften*, vol. I, part A, no. 3, p. 55.

¹¹ Frege always prefers to use on his own account the word '*Grösse*', and employs '*Quantität*' only in discussing the writings of others; but he draws no distinction between the meanings of the two words. '*Grösse*', as employed by him, will here always be translated 'quantity'.

¹² The German word for 'ratio' is '*Verhältnis*', meaning a proportional relation. Since it is not cognate with the word for a rational number, there is not even the appearance of contradiction in the phrase for 'irrational ratio'.

nation; and this same ratio is the same number and can be denoted by the same numerical symbol.

Frege is still interpreting Cantor as using '*b*' to denote a fundamental series, and so he concludes the foregoing passage as follows:

If Illigens understands ratios of quantities (*Größen*), or, what we can now regard as synonymous, real numbers, by the word 'quantities' (*Quantitäten*) as he uses it, and if he means that the symbols for series of numbers do not, on Cantor's theory, denote ratios of quantities, he is right. In Cantor's definition there occur only the fundamental series and the number *b*, and the latter is the symbol for the series of numbers. Nothing is here said about a ratio of quantities. The symbol for the series of numbers simply denotes the fundamental series and accordingly ought not also to denote a ratio of quantities, for it would then be ambiguous.

Here we have Frege's preliminary exposition of the foundation of his theory of how the real numbers should be defined. A word is in place concerning his use of the word 'quantity'. It is a little difficult to state the precise meaning of this word in natural language; but Frege so uses it that a phrase like '2.6 metres' designates a specific quantity of one kind, '5.3 seconds' a quantity of another kind, and so on. He thus takes quantities to be objects, distinct from numbers of any kind. There cannot be two equal quantities, on this use: if two bodies are equal in mass, they have the *same* mass. Quantities fall into many distinct types: masses form one type, lengths another, temperatures a third.

Frege does not use any word corresponding to 'magnitude', in the sense in which one may ask after the magnitude *of* a quantity. It is best compared with nouns like 'cardinality' and 'whereabouts': when one gives the whereabouts of an object, one names a place, not a whereabouts; there are no such objects as whereabouts. The cardinality of a set is given by naming a cardinal number; since one can say what it is for a set to have three, or denumerably many, members without comparing it with any other set, we may say that its cardinality is an intrinsic property. The magnitude of a quantity can be given only as a ratio between that quantity and some other taken as unit: it is therefore an extrinsic property.

It is remarkable that Frege nowhere calls explicit attention in *Grundgesetze* to the fact that, unlike Cantor and Dedekind, he is proposing to define the real numbers without taking the rationals as already known.¹³ In §§ 70–6 he is assessing Illigens's objection that Cantor's real numbers are not quantities, as the rationals are: specifically, that there is no way to explain their use to give the magnitude of a physical quantity. We might therefore wonder why he

¹³ He mentions it explicitly in his letter to Russell of 21 May 1903, saying, 'As it seems to me, you need a double transition: (1) from the cardinal numbers (*Anzahlen*) to the rational numbers, and (2) from the latter to the real numbers generally. I wish to go straight from the cardinal numbers to the real numbers as ratios of quantities.'

troubles to quote and criticise Pringsheim's essay, seeing that this exclusively concerns the *rational*s. The reason is that Frege sees the problem, not as that of explaining how irrational numbers can be used to give the magnitude of a quantity, on the assumption that we already know how the rationals can be used for this purpose, but as that of explaining how real numbers generally, whether rational or irrational, can be so used. In order to assess Illigens's criticism, that Cantor's real numbers cannot be used, as the rationals are used, to give the measure of a quantity, we must first ask in what this use of the rationals consists. That will tell us what, essentially, the rational numbers *are*. When we know this, we shall thereby know what, essentially, the real numbers are, since the primary application of the real numbers is the same as that of the rationals.

It was, for Frege, the same for the rationals as for the natural numbers: the proper way to define them was one that incorporated the principle underlying the salient application that we make of them. The salient type of application of the rationals occurs, on his view, when we say that something is $5/8$ inch long or weighs $3/4$ kilogram. Russell defined the rationals as ratios of integers, and was therefore forced to treat the real number $5/8$ as an object distinct from the rational $5/8$. Frege would, however, have regarded Russell's definition as based on too restrictive a view of the application of the rationals:¹⁴ he saw them as being used, in general, to answer questions that could be meaningfully answered by citing an irrational number. Hence he thought it illegitimate first to define the rationals before introducing irrational numbers: we must ask at the outset how, in general, a real number can serve to give the measure of a quantity.

To answer this question for any specific type of quantity – say temporal durations – we must suppose given an order relation and an operation of addition upon those quantities: it must be known what it is for one duration to be greater than another, and for one to be the sum of two others (namely when a temporal interval whose length is the first is divisible into two subintervals whose lengths are the other two respectively). A crude answer might then be that there is an order-preserving map of the quantities of that type on to the (non-negative) real numbers carrying addition of quantities into addition of numbers. Plainly, this does not yet provide a basis for a definition of real numbers, since such mappings are far from unique: given any such mapping, multiplication by any positive factor will yield another. It is only when we map, not the quantities themselves, but their ratios, that we obtain a unique mapping: the real numbers represent, not the quantities, but their ratios, unless we select a unit quantity and express every other quantity as its ratio to the unit.

As Frege observes, the point is well known; and yet he has occasion, in § 75, to find fault with Cantor for overlooking it. In his article of 1872, Cantor

¹⁴ 'The useless arithmetical ratios are naturally not meant', Frege says in footnote 1 to *Grundgesetze*, vol. II, § 75, after asking in the main text for a definition of 'ratio' in general.

calls both rational numbers and the real numbers he constructs 'numerical magnitudes' (*Zahlengrößen*), an expression he does not use in his *Grundlagen*. In § 2 of the article he explains how his real numbers can be used as measures of distance, and gives the definition:¹⁵

We express this by saying: *The distance from the point a of the point to be determined is equal to b* , where b is the [fundamental] series . . . of corresponding numerical magnitudes.

Frege comments:

In the first place, the mistake is here to be noted, that the unit is nowhere mentioned in the defined expression, although it is necessary for the specification. From this there may arise the delusive appearance that b , b' , b'' , . . . are distances, whereas it can only be a matter of ratios; and such ratios can occur just as well with strengths of electric current, with amounts of mechanical work, etc.

No doubt, if Cantor ever read these comments, he was outraged by the tendentiousness of the criticism, since, although the unit of measurement is left unmentioned in the definition, it is expressly adverted to in the preceding passage in which Cantor set the stage for it. However this may be, Frege was right that the point is crucial for attaining a correct characterisation of the real numbers on the lines he proposed. A correct definition of the *natural numbers* must, on his view, show how such a number can be used to say how many matches there are in a box or books on a shelf. Yet number theory has nothing to do with matches or with books: its business in this regard is only to display what, in general, is involved in stating the cardinality of the objects, of whatever sort, that fall under some concept, and how natural numbers can be used for the purpose. In the same way, analysis has nothing to do with electric charge or mechanical work, with length or temporal duration; but it must display the general principle underlying the use of real numbers to characterise the magnitude of quantities of these and other kinds. A real number does not directly represent the magnitude of a quantity, but only the ratio of one quantity to another of the same type; and this is in common to all the various types. It is because one mass can bear to another the very same ratio that one length bears to another that the principle governing the use of real numbers to state the magnitude of a quantity, relatively to a unit, can be displayed without the need to refer to any particular type of quantity. It is what is in common to all such uses, and only that, which must be incorporated into the characterisation of the real numbers as mathematical objects: that is how statements about them can be allotted a sense which explains their applications, without violating

¹⁵ G. Cantor, 'Über die Ausdehnung eines Satzes aus der Theorie der trigonometrischen Reihen', *Mathematische Annalen*, vol. V, 1872, p. 127, *Gesammelte Abhandlungen*, p. 96.

the generality of arithmetic by allusion to any specific type of empirical application.

Can Cantor explain the applications of real numbers?

These general principles do not in themselves embody any objection to Cantor's theory, or corroborate that of Illigens: we must ask whether the use of real numbers to assign the magnitude of a quantity can be explained on the basis of the theory. In §§ 74–6 Frege considers Cantor's brief retort to Illigens's article,¹⁶ which he finds unclear. He quotes Cantor as there saying, 'It was never asserted by me or by anyone else that the signs b , b' , b'' , ... were concrete magnitudes (*Größen*) in the proper sense of the word. As *abstract objects of thought* (*abstracte Gedankendinge*) they are magnitudes only in the improper or transferred sense of the word.'

Frege surmises that by 'abstract objects of thought' Cantor means what he himself means by 'logical objects', and remarks that, if so, 'there seems to be a good agreement between us on the subject'. Unfortunately, he adds, Cantor fails to define any such logical objects. More important in the present context is Cantor's distinction between concrete magnitudes and magnitudes in the abstract or transferred sense. Presumably, by 'concrete magnitudes' Cantor means particular quantities – areas, masses and the like; but the notion of an abstract magnitude is left woolly. Real numbers are not, for Frege, obtained by considering what Cantor calls 'concrete magnitudes' and abstracting from their specific type: they are obtained as ratios of such concrete magnitudes, in which concrete magnitudes of whatever type may stand to others of the same type.

In his reply to Illigens, Cantor claims that we are in a position to arrive at an exact quantitative determination of concrete magnitudes properly so called, such as geometrical distances, with the help of the abstract magnitudes b , b' , b'' , ...; this, he says, must be regarded as decisive. Thus, Frege comments, 'the application to geometry, far from being a mere agreeable extra, is decisive. But, if it is decisive, this tells against Cantor's theory, because this decisive feature does not occur at all in his definition of numerical magnitude.' The construction of the real numbers comes first in his theory, without reference to their use to assign magnitudes to quantities. 'It is only after the b , b' , b'' , ... have been introduced that the determination of distances by means of numerical magnitudes is given', Frege says in § 75; 'that manner of introducing the numerical magnitudes is purely arithmetical, but does not contain what is said to be decisive; the instructions for determining distances by means of numerical magnitudes contain what is decisive, but are not purely arithmetical. And hence the goal that Cantor has set himself is missed. In the definition

¹⁶ G. Cantor, 'Bemerkung mit Bezug auf den Aufsatz ...', *Mathematische Annalen*, vol. XXXIII, p. 476; *Gesammelte Abhandlungen*, p. 114.

we have the fundamental series on the one hand, and the signs $b, b', b'', \dots$ on the other, and nothing besides'.

This criticism is *not* captious: it contains the core of Frege's objection to Cantor's theory. It resembles that which he may be presumed to have felt to Dedekind's foundations for number theory in *Was sind und was sollen die Zahlen?*: the principle underlying the application, of natural numbers or of real numbers, should not be derived as a corollary, but should be incorporated into the manner in which they are introduced. But, in Cantor's case, there is a further feature. The *general* principle governing applications should be formulated and vindicated; to explain and justify its operation only in a specific type of case, such as distance, is to import something alien into arithmetic.

What would the proper procedure have been, according to Frege? 'The matter would stand differently', he continues in § 75, 'if we had a purely arithmetical or logical definition of *ratio*, from which it could be concluded that there are ratios, and, among them, irrational ones. Then what is decisive would be comprised in this definition, and the determination of a distance by means of a unit and a ratio (a real number) would have only the status of an illustrative example, which could be dispensed with.'

The position, then, is this. Cantor thinks that it tells decisively in favour of his theory that the real numbers he constructs *can* be used to specify distances; Frege thinks that the claim that this is decisive undermines the theory, since no provision for that use has been expressly made in the construction. Frege's argumentation so far does not, however, refute Cantor's claim that his real numbers can be used to give the distance between two points, or sustain Illigens's claim that they cannot. To decide this, the details of Cantor's justification of his claim must be examined.

Illigens had based his criticism primarily upon Cantor's *Grundlagen*, although he also mentions the article of 1872. In his reply, Cantor appeals to the original article, since, in § 2 of that, he had provided an argument to show that his real numbers could be used to give the distance between two points. Frege's account of this is quite accurate. 'It is assumed as known how a distance is determined by a rational number', he says. Cantor considers the distance of the points on a given straight line from some point o on the line chosen as origin, where the points on one side from o are being regarded as having a positive distance from o and those on the other a negative distance. 'If this distance has a rational ratio to the unit of measurement', Cantor says, 'it is expressed by a rational number', thus making the assumption stated by Frege. Hence, as Frege remarks, each term of a fundamental series corresponds to a definite distance and hence to a definite point on the line. 'As the fundamental series proceeds, these points approach without limit a certain point, which is thereby uniquely determined', Frege says. Cantor's own formulation is that, when a point whose distance from the origin does not have a rational ratio to the unit distance, and 'when the point is *known* by a construction, it is always

possible to cite' a fundamental series (1) which 'has to the distance in question such a relation that the points of the straight line to which the distances $a_1, a_2, \dots, a_\nu, \dots$ belong move with increasing ν infinitely near to the point to be determined'. Frege now quotes Cantor's definition, cited above:

We express this by saying: *The distance from the point o of the point to be determined is equal to b* , where b is the series (1) of corresponding numerical magnitudes.

Frege's first objection has already been mentioned, namely that the definition contains no reference to the unit distance; but he acknowledges that this defect could be easily rectified, and then asks:

But which expression is really being defined? It must be assumed as known what the distance of a point from another point is; the so-called numerical magnitudes (b) have already been introduced; and the word 'equal' must also be already known. Thus everything in the defined expression is known, and, if all were in order, the sense of the sentence 'The distance from the point o of the point to be determined is equal to b ' would likewise have to be known, so that a definition would be at least superfluous, and thereby erroneous.

On the face of it, the argument appears unsound. From Cantor's standpoint, he has his real numbers, introduced as determined by fundamental series. He is assuming that we know what it is for the distance of a point from the origin to have a rational ratio to the given unit distance. That does not entitle Frege to say that we know *in general* what the distance of any one point is from any other: what has to be determined is what it is to assign an irrational number to the ratio of the distance of a point from the origin to the unit distance. This is therefore something still apt for definition, which is carried out by reference to a fundamental series which determines that irrational number.

Despite appearances, however, Frege is correct: the definition, as stated, is circular. Given Cantor's assumptions, there is indeed, for each term of a fundamental series, a point whose distance from the origin has that ratio to the unit distance; we might call this sequence of points the 'corresponding series'. The condition we are required to consider is that the points of the corresponding series approach 'infinitely near' the point in question. On the ordinary understanding of 'approach infinitely near', the phrase refers to the distances of the successive terms of the corresponding series from the given point. By assumption, the distance of the given point from the origin does not have a rational ratio to the unit distance; hence neither does the distance of the given point from any term of the corresponding series, since the terms of the fundamental series itself are all rational numbers.

Like his failure to make explicit mention, in the definition, of the unit distance, this piece of carelessness on Cantor's part could easily be remedied. The phrase 'approach infinitely near to the given point' could be replaced by

a formulation in terms of intervals with rational end-points: for any positive rational number r , there is such an interval the ratio of whose length to the unit distance is r , containing the given point and all terms of the corresponding series from some term onwards. Frege, for all the time he spends on Cantor's theory, does not have the patience to locate the inaccuracies precisely, or to consider how they could be corrected.

The fact is that Cantor's whole procedure here is offensive to Frege, in that ratios of distances are defined piecemeal; more exactly, only the second half of the definition is given, the first being assumed as already known. Frege's blanket condemnation of piecemeal definition is sometimes pedantically applied; but, in the present instance, it does not rest on pedantry. Cantor's problem, how to explain the use of irrational numbers as measures, given the use of rational numbers for this purpose, is not for Frege the fundamental one. By taking it as known what is meant by a rational ratio of one distance to another, Cantor has assumed the basic notion requiring analysis: once we have analysed the notion of a ratio of distances, we should have no difficulty in explaining how a real number, whether rational or irrational, can be assigned to such a ratio. As Frege says in § 76, 'These [fundamental] series can serve to determine ratios, but only after we have learned what a ratio of quantities is: and that is precisely what we are lacking'.

Cantor has not only assumed the principal notion to be explained, but has assumed it without sufficient generality. What is required is an explanation, not of the specific notion of a ratio of distances, but of the general notion of a ratio of quantities of some one type: real numbers can then be presented as precisely such a ratio, without importing anything into the definition from outside arithmetic. Because Cantor's construction of the real numbers does not present them as ratios of quantities, he can do no more than illustrate their use to give the magnitude of a quantity case by case; and this has the consequence that he has to appeal to non-arithmetical notions (in his example, to geometrical ones). At the end of § 76, Frege concludes his digression thus:

We have first to know the ratios of quantities, the real numbers; we can then discover how we can determine the ratios by means of fundamental series. It is odd to ascribe to the correlation of the symbols $b, b', b'', \dots$ any creative power. Bringing geometry in is therefore decisive, since by doing so one gains hold of that content which takes all the strain. But then what is decisive belongs to geometry, and Cantor's theory is by no means purely arithmetical.

CHAPTER 22

Frege's Theory of Real Numbers

The concept of quantity

By the end of section (f) of Part III.1 of *Grundgesetze* it has been fully established that Frege is proposing to define the real numbers, positive and negative, as ratios of quantities. The last section (g), comprising §§ 160–4, sketches in outline how he intends to explain the notion of a ratio of quantities. The first question is naturally what a quantity is. This, he claims, has never yet been satisfactorily stated. 'When we scrutinise the attempted definitions, we frequently come upon the phrase "of the same type" or the like. In these definitions, it is required of quantities that those of the same type should be able to be compared, added and subtracted, and even that a quantity be decomposable into parts of the same type.'¹ To this Frege objects that the phrase 'of the same type' says nothing at all: 'for things can be of the same type in one respect, which are of different types in another. Hence the question whether an object is of the same type as another cannot be answered "Yes" or "No": the first demand of logic, that of a sharp boundary, is unsatisfied.'

'Others', Frege continues, 'define "quantity" by means of the words "greater" and "smaller", or "increase" and "diminish"; but nothing is thereby achieved, for it remains unexplained in what the relation of being greater, or the activity of increasing, consists.' The same goes for words like 'addition', 'sum', 'reduplicate' and 'synthesis';² 'when one has explained words in a particular context, one ought not to fancy that one has associated a sense with them in other contexts. One here simply goes round in a circle, as it seems, by always defining one word by means of another which is equally in need of definition, without thereby coming any closer to the heart of the matter.'

The mistake underlying all these attempts consists, Frege says in § 161, in posing the question wrongly. The essential concept is not that of a quantity, but of a *type* of quantity, or, as he prefers to say, a quantitative domain

¹ Frege here refers to Otto Stolz as an example.

² The last of these Frege quotes from Hankel.

(Grössengebiet):³ distances form one such domain, volumes form another, and so on. 'Instead of asking, "What properties must an object have in order to be a quantity?"', Frege says, 'we must ask, "What must be the characteristics of a concept for its extension to be a quantitative domain?"'; something is a quantity, not in itself, but in virtue of belonging, with other objects, to a class constituting a quantitative domain.

Quantitative domains

§ 162 opens with the abrupt declaration that, to simplify the construction, 'we shall leave absolute quantities out of account, and concentrate exclusively on those quantitative domains in which there is an inverse', that is, which contain positive and negative quantities. Temporal distances provide a natural example of the latter, in that they have a direction; temperatures provide a good instance of the former, since, while they have a natural zero, there can be no temperature lower than absolute zero. Given a domain of absolute quantities, we can indeed always associate with it a domain of signed ones, 'by considering e.g. one gramme as + one gramme, i.e. as the relation of a mass m to a mass m' when m exceeds m' by one gramme', as Russell and Whitehead put it; and, as they continue, given a zero, we can get back to the absolute domain, since 'what is commonly called simply one gramme will . . . be the mass which has the relation + one gramme to the zero of mass'.⁴ Frege, however, does not even trouble to offer this much of an explanation. The restriction impairs his claim to give a comprehensive analysis of the concept of quantity, as also does his neglect of cyclic domains such as the domain of angles;⁵ the magnitudes of all these, relative to a unit, are after all also given by real numbers.

Frege immediately quotes an extensive passage from Gauss.⁶ This discusses the conditions under which positive and negative integers may be assigned to elements of some totality. Gauss says that the integers must be assigned, not to objects, but to relations on an underlying set of objects with a discrete linear ordering, unbounded in both directions. The relations are those any one of which any object in the set has to another separated from it in a specific direction in the ordering by a specific number of intervening objects; thus these relations are closed under composition and inverse, and include the identity relation as a zero, and form, in fact, a group of permutations.

Frege seizes upon these suggestions as supplying the main features of his

³ The term 'quantitative domain' appears very early in Frege's writings, with essentially the same meaning, namely in his Habilitationsschrift of 1874, *Rechnungsmethoden, die sich auf eine Erweiterung des Grössenbegriffes gründen*.

⁴ A.N. Whitehead and B. Russell, *Principia Mathematica*, vol. III, 1913, part VI, 'Quantity', p. 233.

⁵ Dealt with by Russell and Whitehead in section D of their part VI.

⁶ C.F. Gauss, review of his own 'Theoria residuorum biquadraticorum: Commentatio secunda', *Werke*, vol. II, Göttingen, 1863, pp. 175–6.

characterisation of a quantitative domain. All the persuasive skill he showed in *Grundlagen* and elsewhere in convincing readers that he had given the correct analysis of intuitive concepts here deserts him. He was of course entirely right in insisting that the concept to be explained is that of a quantitative domain, not that of an individual quantity; but those at whom he jeered in § 160 were quite right to seize on the addition and comparability of quantities of a given type as central features, whether those quantities are absolute or distinguished as positive and negative. It is essential to a quantitative domain of any kind that there should be an operation of adding its elements; that this is more fundamental than that they should be linearly ordered by magnitude is apparent from the existence of cyclic domains like that of angles. The point was put very forcefully in Frege's Habilitationsschrift of 1874. He first remarks that 'one will not give a beginner a correct idea of an angle by placing a drawing of one before him . . . One shows [him] how angles are added, and then he knows what they are.'⁷ He subsequently generalises the point, saying that 'there is so intimate a connection between the concepts of addition and of quantity that one cannot begin to grasp the latter without the former'.⁸

We know, then, that there must be defined on any quantitative domain, in the general sense that includes absolute and cyclic ones, an operation playing the role of addition, and, on most such domains, a linear ordering playing the role of an ordering by magnitude; but we do not yet know which operation and which relation these will be, nor which objects can be elements of a quantitative domain. Frege, however, proceeds immediately to offer answers to the first and third of these questions; the second, concerning the ordering relation, receives a corresponding answer in Part III.2. Because he has decided to confine himself to quantitative domains containing negative quantities, he follows Gauss in requiring such a domain to consist of permutations of some underlying set and in taking the addition operation to be composition, under which the domain is closed; since it will also be closed under inverse, it will be a group of permutations, and, when the ordering is suitably defined, an ordered group. (Frege nowhere uses the term 'group' in *Grundgesetze*, although he must have been familiar with it).⁹

This falls very far below Frege's usual standards of conceptual analysis. It could be argued that 'quantitative domain' should be understood as a purely structural term, on the ground that any group that has the right group structure, as subsequently analysed by Frege, will admit application of the notion of ratio as a relation between its elements, and an assignment of real numbers to those ratios, whatever those elements may be, and whatever the group operation is. But this is not Frege's position: he requires the elements to be permutations

⁷ *Rechnungsmethoden*, p. 1. (See note 3.)

⁸ *Ibid.*, p. 2.

⁹ For example, from the second volume of Heinrich Weber's *Lehrbuch der Algebra*, which appeared in 1896.

and the group operation to be composition, although he leaves the underlying set uncharacterised; but he offers no good argument for the requirement. Group-theoretically, there is no loss of generality, since every group is isomorphic to a group of permutations; but since these are, in general, permutations on the elements of the original group, this is not explanatory. The question is precisely on what underlying set the permutations Frege identifies as elements of a quantitative domain operate. In view of the generality required, this cannot be specified in the formal definition; but we need to have an idea what that set will be, in representative cases, before we can accept or even understand Frege's analysis of the notion of quantity. When the domain consists of spatial or temporal distances, there is no problem: the underlying set is naturally taken to comprise points or instants. What, however, when the domain consists of masses? The suggestion of Whitehead and Russell, as it stands, represents signed masses as permutations on absolute masses; if we follow it, we need to know what a domain of absolute quantities is before we can know what a domain with positive and negative quantities is. It might be proposed that the underlying set should be taken to consist of the physical objects to which absolute masses are assigned. We could not then assume, however, that the group of permutations with which Frege identifies the quantitative domain contained all the elements it was required to have to be a quantitative domain on his definition: it is not true a priori that, for every conceivable mass, there is an object that has that mass.

Frege has thus not achieved a convincing analysis of the concept of a quantitative domain. His illustration, in § 163, does not greatly help: it is the usual one, used by Veronese, Hölder and Cantor, of distances along a straight line; the underlying set comprises its points, and the permutations forming the quantities of the domain are displacements along it. The example shows, indeed, that some quantitative domains conform to Frege's model; it is powerless to show that all can be so characterised. An adequate general characterisation of the notion of quantity would pay much more attention to how it is applied in practice; it would also embrace absolute domains, cyclic domains, and domains of vectors of more than one dimension. Frege is so anxious to press on to his definition of real numbers that he ignores all quantitative domains save those that have the structure of the real line; as a result, he offers a highly defective analysis of the concept on which he fastens so much attention. Possibly this deficiency would have been corrected in a Part IV which never saw the light of day.

What would not have been corrected was the philosophical naivety of taking it for granted that every quantity has a precise value representable by the assignment to it of a real number relatively to a unit but discoverable by us only to within an approximation. We are led to adopt this picture by devising ever more accurate methods of measurement; but with what right do we assume that its limit is a point, and not an interval, or at least that it is an

interval with precise end-points, rather than with fuzzy ones? It would be absurd to say that we impose the system of natural numbers upon reality; but it is not at all absurd to say the same about the mathematical continuum. We are not given physical reality as a set of instantaneous states arrayed in a dense, complete ordering: we apprehend it only over temporal intervals. The idea of discontinuous change is not, of itself, conceptually abhorrent; we commonly think of ourselves as experiencing it, as when darkness succeeds illumination when the light is switched off. More exact examination shows that such changes, at the macroscopic level, are in fact continuous; but that does not make the idea of such simple discontinuities absurd. We could, for instance, understand the idea that the colour of a surface might abruptly change from, say, red to green. What is conceptually absurd is to apply to such a change the distinction that can be made with Dedekind cuts, asking what colour the surface was at the instant of change: there are not two distinct possibilities, according as it was then red or then green. Yet more absurd would be the idea of the surface's being red through an interval, save at one particular moment, when it was green. These are not *physical* absurdities, violating well known laws of physics: they are much deeper absurdities, *conceptual* absurdities. And they suggest that the mathematical continuum fits physical reality somewhat imperfectly, yielding apparent logical possibilities that are no possibilities at all. We are familiar with the thought that quantities obtained by differentiation, like velocity and acceleration, do not possess their values at any particular moment in logical independence of what their values are at all other moments; but the foregoing examples suggest that the same is true of *all* quantities, even the fundamental ones, so that these are not 'loose and separate', as Hume absurdly said. But, if so, the mathematical continuum is not the correct model for physical reality, but only one we use because we do not have a better. In regarding real numbers as 'measurement-numbers', Frege was treating of a wholly idealised conception of their application, instead of giving an analysis of our actual procedures of measurement and their underlying assumptions. By doing so, he skimmed the task he had set himself.

Hölder

Frege was not as out of step with other mathematicians as he imagined. Only two years before the second volume of *Grundgesetze* appeared, Otto Hölder published an article treating of much the same topic as Part III of that work.¹⁰

¹⁰ O. Hölder, 'Die Axiome der Quantität und die Lehre vom Mass', *Berichte über die Verhandlungen der Königlich Sächsischen Gesellschaft der Wissenschaften zu Leipzig: mathematische und physikalische Klasse*, vol. 53, 1901, pp. 1–64. It was this Otto Hölder after whom the Jordan-Hölder theorem is (in part) named. In his article, Hölder does not mention Frege, but expresses himself as of the same opinion as he in regarding arithmetic as purely logical. He is, however, quite unaware of the advances in logic that Frege had pioneered, and remarks in a footnote (p. 2, fn. 1) that arithmetical proofs cannot be rendered in any existing logical calculus.

A comparison between them is extremely instructive. Hölder is aiming at a general theory of measurable quantity. He is as explicit as Frege about the need for generality, and criticises earlier work by Veronese for failing to separate the general axioms of quantity from the geometrical axioms governing segments of a straight line.¹¹ Hölder characterises absolute quantitative domains, without a zero quantity; he does so axiomatically in terms of an operation of addition, assumed associative, and a linear ordering relation, assumed dense, complete and left- and right-invariant, both taken as primitive. Such a domain is then an ordered upper semigroup, although, like Frege, Hölder does not use explicit group-theoretic terminology. He appears to have been the first to give a correct proof of the archimedean law from the completeness of the ordering, and also to prove the commutativity of addition from the archimedean law. As we shall see, Frege obtained similar theorems in his Part III.2; but Frege's theorems are more powerful than those of Hölder, because his assumptions are considerably weaker.¹²

For n a positive integer, and a a quantity, Hölder easily defines the multiple na in terms of addition. He proceeds to characterise the notion of a ratio between two quantities, and associates a real number with every such ratio. Unlike Frege, however, he does not *construct* the real numbers by this means. Rather, he first defines the positive rational numbers, in effect as equivalence classes of pairs of positive integers.¹³ He then takes the real numbers to be defined by Dedekind's method, which he sets out without Dedekind's own appeal to mathematical creation, identifying the real numbers with the corresponding cut in the rational line in which the lower class has no greatest element.¹⁴

The correct definition of ratio, given addition and therefore multiples, was well known, having been framed by Euclid,¹⁵ and Hölder appeals expressly to it; it allows the comparison of ratios between pairs of elements of different domains, provided each has an operation of addition, but Hölder confines himself to comparisons within a single domain. Intuitively, we shall want to associate the rational number n/m with the ratio of a to b when $ma = nb$. Euclid defines a as having the same ratio to b as c has to d when, for all positive integers n and m , ma is smaller than (equal to, greater than) nb if and

¹¹ Op. cit., p. 37, fn. 1; see G. Veronese, 'Il continuo rettilineo e l'assioma V d'Archimede', *Atti della Reale Accademia dei Lincei*, series 4, memorie della classe delle scienze fisiche, matematiche e naturali, vol. 6, 1889, pp. 603–24.

¹² If the conjecture that vol. II of *Grundgesetze* was already written when vol. I was published is correct, Frege could have had the priority if he had published sooner; but the mathematical community would not have accorded it to him, because nobody troubled to read vol. II.

¹³ He actually says, rather vaguely, that all equivalent pairs 'represent, in accordance with our (arbitrary) interpretation, an object which we designate a *rational number*' (op. cit., p. 20).

¹⁴ The phrase used is again slightly vague: a cut 'can be regarded as representing' a rational or irrational number, and, in the first case, 'identified with it straight out', and, in the second, 'called an irrational number straight out'; op. cit., p. 22.

¹⁵ Euclid, *Elements*, book V, definition 5.

only if mc is smaller than (equal to, greater than) nd .¹⁶ Hölder's contribution is to notice the close connection between the ideas of Euclid and of Dedekind.¹⁷ For, in view of the archimedean law, every ratio between quantities determines a Dedekind cut in the rational line, and hence has the real number corresponding to that cut associated with it.

In the second part of his paper, Hölder applies his theory to everybody's favourite example, of directed segments of a straight line. The interest of the example, for him, lay in its indicating how to handle dual domains of opposite quantities, together forming a domain of positive and negative quantities of the kind Frege concerned himself with; but we need not follow the details of Hölder's treatment.

It is a matter for the deepest regret that neither Frege nor Hölder ever became aware of the other's work. Had he had to comment on Hölder's theory in his Part III.1, he could not have dismissed it so lightly as he in fact dismissed Dedekind's theory: it shows very clearly how that theory can be applied to ratios of quantities. In doing so, it also brings out more sharply than before the exact nature of Frege's objection to such a theory as Dedekind's. Hölder, like everyone else except Frege, first defines the rationals, essentially as ratios between positive integers, and then defines the real numbers in terms of them. For that reason, although the principles underlying the use both of rationals and of irrationals to give the magnitude of a ratio between quantities are very direct, they are still external to the definitions of the numbers themselves. Frege, by insisting that rationals and irrationals should be defined together, made it necessary that that application of them be internal to their definition. Put in that way, the difference between Frege and Dedekind, once we set aside the matter of free creation by the human mind, becomes much narrower than one might suppose from Part III.1 of *Grundgesetze*. There is a significant methodological difference: for Frege, the theory of quantity is an integral part of the foundations of analysis, not a mere addendum of interest primarily to applied mathematicians. But the mathematical difference becomes more slender. In particular, if he had reached the point in Part III.2 of defining ratios, Frege would have had to use the Euclidean definition, or something very like it, and would thus have come quite close to Dedekind's conception of the real numbers.

The existence of a quantitative domain

In § 164, which concludes Part III.1, Frege resolves the doubt expressed in § 159. In order to ensure the existence of the real numbers, at least one quantitative domain must be proved to exist, containing quantities bearing

¹⁶ The bracketed expressions occur in Euclid's definition, but are here superfluous.

¹⁷ In the note to p. 29, Hölder very properly points out that Dedekind himself acknowledged the affinity in the Preface to *Was sind und was sollen die Zahlen?*

irrational ratios to one another; for, if it did not, the real numbers, defined as ratios of elements of such a quantitative domain, would all be equal to one another and to the null relation. Furthermore, the proof must use only logical resources. As in all cases, the domain will consist of permutations on an underlying set. Frege observes that the set underlying such a domain must have a cardinality higher than the class of natural numbers; he mentions the fact (not proved in Part II) that the number of classes of natural numbers is greater than the number of natural numbers, but fails to make any acknowledgement to Cantor.¹⁸ He therefore proposes to use classes of natural numbers in specifying the underlying set.

If we temporarily assume the irrational numbers known, Frege continues, we can regard every positive real number a as representable in the form

$$r + \sum_{k=1}^{k=\infty} 2^{-n_k}$$

where r is a non-negative integer, and $n_1, n_2, \dots$ form an infinite monotone increasing sequence of positive integers. This amounts to giving the binary expansion of a (in descending powers of 2, as a decimal expansion is in descending powers of 10); the expansion is chosen to be non-terminating, so that $1/2$ is represented by the infinite series $1/4 + 1/8 + 1/16 \dots$ Thus to every positive number a , rational or irrational, is associated an ordered pair, whose first term is a non-negative integer r and whose second term is an infinite class of positive integers (which suffices to determine the sequence); these may be replaced respectively by a natural number and an infinite class of natural numbers not containing 0.

This, then, is the underlying set; the permutations on it are to be defined in some such way as the following. For each positive real number b there is a relation holding between other positive real numbers a and c just in case $a + b = c$. This relation can be defined, Frege says, without invoking the real numbers a, b and c , and thus without presupposing the real numbers. He does not here give the definition; the following should serve the purpose. Suppose given an ordered pair $\langle s, B \rangle$, where s is a natural number and B an infinite class of natural numbers not containing 0: we want to define a relation between similar such pairs $\langle r, A \rangle$ and $\langle t, C \rangle$. Let us first say that a natural number n is *free* if, for every $m > n$ such that m belongs both to A and to B , there is a number k such that $n < k < m$ belonging neither to A nor to B . We may then say that our relation holds if the following two conditions are fulfilled:

- (i) for each n , n belongs to C if and only if n is positive and either is free

¹⁸ This omission is truly scandalous; Frege would never have displayed such ill manners at the time of writing *Grundgesetze*.

and belongs to one of A and B but not the other, or is not free and belongs either to both A and B or neither;

(ii) $t = r + s$ if 0 is free, and $t = r + s + 1$ if 0 is not free.

This definition is intended to determine the relation as holding between $\langle r, A \rangle$ and $\langle t, C \rangle$ just in case $a + b = c$, where $\langle r, A \rangle$, $\langle s, B \rangle$ and $\langle t, C \rangle$ intuitively represent the real numbers a , b and c respectively. As Frege observes, we now have such relations corresponding to every pair $\langle s, B \rangle$; taken together with their inverses, these correspond one to one to the positive and negative real numbers; and to the addition of the numbers b and b' corresponds the composition of the corresponding relations. 'The class of these relations (*Relationen*)', he says, 'is now a domain that suffices for our plan', but adds that 'it is not thereby said that we shall hold precisely to this route'.

He could not hold precisely to it, because, in the coming series of formal definitions, he requires a quantitative domain to consist of permutations on an underlying set; that is to say, he requires the relations it comprises to be one-one, all to be defined on the same domain and to have a converse domain identical with their domain. The relations mentioned in § 164, and formally defined above, are not, however, permutations: the operation of adding the positive real number b carries the positive real numbers into the real numbers greater than b . In Volume II, he does not reach the formal proof of the existence of a quantitative domain. If, when he did, he had wanted to use additive transformations, he would have had to take the underlying set to be isomorphic to all the real numbers, positive, negative and 0, which would have been somewhat more complicated; if he had wanted the underlying set to be isomorphic just to the positive reals, he would have had to use multiplicative transformations, which would have been very much more complicated to define with the resources available. There is, of course, no actual doubt that either could be done.

The formal treatment

When the reader comes to the formal development in Part III.2, much has been settled. The first problem is to characterise a quantitative domain; and he knows that it must be an ordered group of permutations satisfying a number of conditions. The mathematical interest of the work is considerable; it is a thoroughgoing exploration of groups with orderings, yielding, as already noted, theorems more powerful than those proved by Otto Hölder in the paper discussed above. The interest is not due to Frege's ultimate purpose: he could simply have laid down all the conditions he wanted a quantitative domain to satisfy and incorporated them in a single definition. The interest is due, rather,

to Frege's concern for what we should call axiomatics, that is, for intellectual economy: as he explains in § 175, he wants to achieve his aim by making the fewest assumptions adequate for the purpose, ensuring that those he does make are independent of one another. Hence, although a quantitative domain will prove in the end to be a linearly ordered group in the standard sense, in which the ordering is both left- and right-invariant, many theorems are proved concerning groups with orderings not assumed to be linear or to be more than right-invariant.

Before we proceed further, a word is in place concerning Frege's formal apparatus. A reader unfamiliar with it may have felt uncertain whether his quantitative domains contain objects, relations or functions. The answer is that they contain objects, but objects which are extensions of relations. The formal system of *Grundgesetze* contains expressions for functions both of one and of two arguments; these include both one-place and two-place predicates, that is, expressions both for concepts and for binary relations (*Beziehungen*). There is, however, no special operator for forming terms for value-ranges of functions of two arguments: this is accomplished by reiterated use of the abstraction operator (symbolised by the smooth breathing on a Greek vowel)¹⁹ for forming terms for value-ranges of functions of a single argument. Thus ' $\acute{\epsilon}(\epsilon + 3)$ ' denotes the value-range of the function that maps a number x on to $x + 3$; and so ' $\acute{\alpha}\acute{\epsilon}(\epsilon + \alpha)$ ' denotes the value-range of the function that maps a number y on to $\acute{\epsilon}(\epsilon + y)$. This 'double value-range' is then taken by Frege as the extension of the binary function of addition. In the same way, ' $\acute{\epsilon}(\epsilon < 3)$ ' denotes the class of numbers less than 3, while ' $\acute{\alpha}\acute{\epsilon}(\epsilon < \alpha)$ ' denotes the value-range of the function that maps a number y on to the class of numbers less than y . This, being the double value-range of a relation (*Beziehung*), in this case the 'less-than' relation, is identified by Frege with its extension, standing to it as a class to a concept; the extension of a relation, being an object, is called a *Relation*, to distinguish it from a relation proper. This is just an example of how, throughout *Grundgesetze*, Frege is able to work with value-ranges in place of concepts, relations and functions. A quantitative domain contains *Relationen* – extensions of relations – rather than relations in the true sense: specifically, extensions of one-one relations on an underlying set. We may, for brevity, call these 'permutations'; throughout Part III.2, Frege works exclusively with value-ranges of various kinds, concepts, relations and functions hardly ever making an appearance. For this reason, the word 'relation' itself will henceforth be understood in the sense of '*Relation*', namely as applying to the extension of a relation (*Beziehung*) in the proper sense.

Frege begins by announcing that addition – that is, composition of permu-

¹⁹ Some commentators on Frege write the smooth breathing over Greek consonants, which looks extremely odd. Of course, there is no logical mistake; but Frege never used Greek consonants as bound individual variables, and it would surely have offended his sense of propriety to write a breathing over them if he had.

tations – in a quantitative domain must satisfy the commutative and associative laws. He then proves that composition of relations is always associative.²⁰ It is by no means always commutative, as he remarks. A special case in which it is is first singled out by Frege, namely the class consisting of a relation p together with all its iterations $p|p, p|(p|p), \dots$ (Here the symbol $|$ is used for composition, in place of Frege's own; no attempt will be made to reproduce his symbolism.) Frege uses his definition of the ancestral to express membership of this class without reference to natural numbers (and hence to multiples of the form np as used by Hölder). Even when p is a permutation, the class of its positive multiples will not always be a group. In this connection, Frege defines an important notion, that of the *domain* of a class P of relations. This consists of P together with the identity and the inverses of all members of P . If P is the class of all multiples of a permutation p , its domain will of course be the cyclic group generated by p ; but it should be noted that the domain of a class of permutations will not always be the group generated by it, since it is not required to be closed under composition.

The next problem is how to introduce the notion of order. Frege chooses to do it by defining the conditions for a class to consist of the positive elements of a group of permutations on which there is an ordering, and defining the ordering in terms of that class. His first approach is to introduce the notion of what he calls a *positival class*. A positival class is a class of permutations on some underlying set satisfying the following four conditions:

- (1) if p and q are in P , so is $p|q$;
- (2) the identity e is not in P ;
- (3) if p and q are in P , then $p|q^-$ is in the domain of P ;
- (4) if p and q are in P , then $p^-|q$ is in the domain of P .

Here ' p^- ' denotes the inverse of p . If P is a positival class according to the foregoing definition, the domain of P will be the group generated by P . Frege goes on to introduce an order relation on the group by setting p less than q if and only if $q|p^-$ is in P . It follows immediately that the order relation $<$ thus defined is right-invariant,²¹ that is, that if $p < q$, then $p|r < q|r$ for any element

²⁰ Composition of relations was defined in vol. I, § 54.

²¹ Frege's permutations are one-one relations, not functions, and his symbol for composition is defined like Russell's relative product: if x stands to y in the p -relation iff x is the father of y , and in the q -relation iff x is the mother of y , then x stands to y in the $p|q$ -relation iff x is the maternal grandfather of y . In standard group-theoretical notation, this would be written qp , the symbol for the operation to be applied first being written first. Using that notation, one would say that Frege defined his order relation to be *left-invariant*; but it seems less confusing to stick to a notation that accords with Frege's in respect of the order in which the variables are written.

r of the group, and, further, that P is the set of elements of the group greater than the identity e (the set of positive elements). Furthermore, it follows easily from (1) and (2) that $<$ is a strict partial ordering of the group (i.e. is transitive and asymmetrical).

Frege is, however, extremely worried that he is unable to establish whether or not condition (4) is independent of the other three. In fact, it is;²² Frege, uncertain of the point, proceeds to prove as much as he can, from § 175 to § 216, without invoking clause (4), and calls attention, in § 217, to the fact that at that stage he finds himself compelled to do so.

If clause (4) does not hold, the domain of P will not constitute the whole group generated by it, which will in fact be the domain together with the elements $p^{-1}q$ for p and q in P . We may nevertheless still consider the order relation as defined over the whole group. Clause (3) in effect says that $<$ is a strict linear ordering of P , and is equivalent to the proposition that it is a strict upper semilinear ordering of the group. This means that it is a strict partial ordering such that the elements greater than any given one are comparable, and that, for any two incomparable elements, there is a third greater than both of them: pictorially, it may branch downwards, but cannot branch upwards. Clause (4) says that $<$ is a strict linear ordering of the negative elements (those less than e), and is equivalent to the proposition that $<$ is a strict lower semilinear ordering of the group (where this has the obvious meaning). (3) and (4) together are therefore tantamount to the proposition that $<$ is a strict linear ordering of the group. If the ordering is left-invariant, clause (4) must hold, since, if $p < q$, by left-invariance $e < p^{-1}q$, i.e. $p^{-1}q$ is in P . (The converse, however, does not hold: a group may have a right-invariant linear ordering that is not left-invariant.) Frege's independence problem thus amounts to asking whether there is a group with a right- but not left-invariant upper semilinear ordering that is not linear. Since in fact there is, the theorems that he takes care to prove without invoking clause (4) hold for a genuine class of groups.

The notion of a positival class was only a preliminary approach to that which Frege wants, namely that of a *positive* class. This is a positival class P such that the ordering $<$ is dense and complete. To characterise the notion of completeness, Frege has of course to define the notion of the least upper bound of a subclass A of P . His definition does not agree with what appears to us the obvious way of defining the notion. He uses as an auxiliary notion what we might call that of an 'upper rim' of the class A : r is an upper rim of A in P if and only if A contains every member of P less than r (Frege gives no verbal rendering of this notion, but only a symbol). What he calls an 'upper bound' (*obere Grenze*) or simply 'bound' of A in P is now defined to be an

²² See S.A. Adeleke, M.A.E. Dummett and Peter M. Neumann, 'On a Question of Frege's about Right-Ordered Groups', *Bulletin of the London Mathematical Society*, vol. 19, 1987, pp. 513–21, theorem 2.1.

element r of P which is an upper rim of A in P and is not less than any other upper rim of A in P which belongs to P . Since $<$ linearly orders P , there can be at most one upper bound, in this sense, of a class A : it is the greatest lower bound, in our sense, of the complement of A . If A is such as to contain every element of P smaller than any element it contains, Frege's upper bound of A will be its least upper bound in the usual sense. Frege's formulation of the condition for $<$ to be complete in P is that, if some member of a class A is an upper rim of A in P , but there is an element of P not in A , then some member of P is an upper bound of A in P .

Frege continues his policy of avoiding appeal to clause (4) even after introducing the notion of a positive class. Oddly, he does not raise the question whether clause (4), if independent of clauses (1), (2) and (3), remains independent after the addition of the assumptions of completeness and density; as we shall see, it does not. Frege is concerned with the archimedean law, that, for any positive elements p and q , there is a multiple of p which is not less than q ; he formulates it with the help of the class of multiples of an element mentioned above. The most important theorems that he proves are as follows:

Theorem 635 (§ 213). If $<$ is a complete upper semilinear ordering, then the archimedean law holds.

Hölder had derived the archimedean law from the completeness of the ordering in his paper of two years earlier, but he was using considerably stronger assumptions than Frege's, namely that the ordering is dense, left-invariant and linear. The completeness of the ordering is needed to obtain the real numbers; but it is the archimedean law that is important in the subsequent theorems.

Frege employed, though did not name, an interesting and fruitful concept, namely that of a restricted kind of left-invariance which we may express as the ordering's being 'limp' ('left-invariant under multiplication by positive elements'). The ordering has this property if, whenever $q < r$, and p is positive, then $p|q < p|r$. The next theorem uses this notion.

Theorem 637 (§ 216). If $<$ is an upper semilinear, archimedean ordering, then $<$ is limp.

These two theorems have been so expressed in virtue of Frege's avoidance, in their proofs, of appeal to clause (4). The next two theorems do appeal to it.

Theorem 641 (§ 218). If $<$ is a linear, archimedean ordering, then $<$ is left-invariant.

Theorem 689 (§ 244). If $<$ is a dense, linear, archimedean ordering, then the group is abelian: that is, the commutative law holds for composition.

Hölder also derived commutativity from the archimedean law, but he had to assume left-invariance, whereas, for Frege, left-invariance was automatic by Theorem 641. The assumption of density is unnecessary; but Frege's appeal to it in his proof is not a fault, since different proofs are needed for the two cases.

With the help of Frege's theorem 637, a further improvement can be obtained, namely the

Theorem. If $<$ is an archimedean, upper semilinear ordering, $<$ is linear and the group is abelian.²³

Thus clause (4) is no longer independent in the presence of the assumption of completeness, or even just of the archimedean law, which then suffices to prove commutativity.

With theorem 689, Frege reached the end of the quest for a proof of the commutative law announced at the very beginning of Part III.2, and therewith the end of Volume II (save for the Appendix on Russell's paradox). A quantitative domain, in the narrow sense, could now with assurance be identified with the domain of a positive class.

In his brief concluding § 245, Frege announces as the next task to prove the existence of a positive class, along the lines indicated in § 164. That, he says, will open up the possibility of defining real numbers as ratios of quantities belonging to the domain of the same positive class. 'And we shall then also be able to prove that the real numbers themselves belong as quantities to the domain of a positive class.'

The missing conclusion of Part III.2 would have been laborious, but would have presented no essential difficulties. The device of § 164 would have had to be amended a little; but this would have required nothing but work. Frege would have had essentially to use Euclid's definition of when the ratio of a quantity p to another quantity q of some domain D coincided with that of a quantity r to a quantity s , both belonging to a domain E , whether the same as D or distinct from it. He would not have defined a phrase containing 'the same' or 'coincides with', but would have defined an equivalence relation between ordered pairs of quantities. (He had defined an ordered pair in Volume I, § 144, as the class of relations in which the first term stood to the second.) Nor, when he had hitherto refrained from appealing to the natural numbers in characterising multiples of quantities, would he have been likely to start doing so at this point; but his definition would of necessity have been

²³ See S.A. Adeleke, M.A.E. Dummett and Peter M. Neumann, op. cit., theorem 3.1.

essentially Euclid's, all the same. This definition would give the criterion of identity for ratios; we might therefore naturally expect Frege then to define the real numbers by logical abstraction, i.e. as equivalence classes of ordered pairs of quantities. This, however, would not yield the result he demands in his last sentence, that the real numbers should themselves form a quantitative domain, because they would then have to be extensions of relations, which are not, for Frege, classes of ordered pairs, but double value-ranges. He would therefore have had to use a variation of the method. A real number would have to be the relation between a quantity r and a quantity s of the same domain which obtained when r stood to s in the same ratio as some fixed quantity p stood to another fixed quantity q of the same domain, i.e. when the pair $\langle r, s \rangle$ stood in the relevant equivalence relation to the pair $\langle p, q \rangle$.

If we imagine the axioms governing value-ranges to be quite different, yielding a consistent theory analogous to ZF set theory, and Frege's notion of an ordered pair replaced by the modern one, there would be no trouble about any of the work in Volume II, Part III, and none about the proof of the existence of a positive class. The definition of the real numbers as ratios would, however, be blocked, because their domain, as relations, would be the union of all domains of positive classes, and the class of such domains would certainly be a proper class. This, of course, was precisely the fate of Frege's definition of cardinal numbers, including the natural numbers. The paradoxes of set theory imposed limits quite unexpected by him upon definition by logical abstraction.

CHAPTER 23

Assessment

How should we evaluate Frege's philosophy of mathematics? Strictly speaking, he did not have a philosophy of mathematics: he never enunciated general principles applicable to all branches of mathematics, or to all branches save geometry; he never claimed to have more than a philosophy of arithmetic. In this he does not compare very unfavourably with others, Hilbert for example. What he lacked in scope, he made up for in breadth of coverage and in precision. We are usually too impressed with the really creative ideas of Hilbert or of Brouwer to pay much attention to the patchy or unconvincing soil in which they are rooted. We pass over Hilbert's sloppy account of the constitution of the natural numbers and the content of finitistic mathematics, and readily forgive him his failure to make precise the notion of a finitistic proof. We overlook the inadequacy of Brouwer's repeated explanations of the genesis of the natural-number sequence, and ignore his solipsism and his failure to achieve a coherent account of the relation between mental constructions and their symbolic formulations. In Frege's writings, by contrast, everything is lucid and explicit: when there are mistakes, they are set out clearly for all to recognise.

Frege had answers – by no means always the right answers, but invariably definite answers – to all the philosophical problems concerning the branches of mathematics with which he dealt. He had an account to offer of the applications of arithmetic; of the status of its objects; of the kind of necessity attaching to arithmetical truths; and of how to reconcile their a priori character with our attainment of new knowledge about arithmetic. His view of the status of the numbers, ontological and epistemological, proved to be catastrophically wrong; for the last nineteen years of his life, he himself acknowledged it to have been wrong, and regarded that as bringing with it the collapse of his entire philosophy of arithmetic. In spite of efforts like those of Crispin Wright to defend it, we can clearly see that his view of this question was in error: but we have not supplied any very good alternative. In answering the remaining questions, we have not, save in one crucial respect, advanced very far beyond Frege at all.

The application of mathematics

Most philosophies of mathematics either ignore its applications, or have a very lame account to give of them. Some writers exclaim at the 'miraculous' nature of such applications¹ – Riemannian geometry and general relativity form a favourite example – and some attempt explanations in terms of the evolutionary advantages of an accord between human patterns of thought and the structure of reality. Frege's objective was to destroy the illusion that any miracle occurs. The possibility of the applications was built into the theory from the outset; its foundations must be so constructed as to display the most general form of those applications, and then particular applications will not appear a miracle.

Frege did not in practice carry out his own principles in this regard with complete success. He failed to provide a sufficiently general analysis of domains of measurable quantities, or a justification of the analysis he gave; and he failed to explore the physical and metaphysical presuppositions underlying the assumption that such a domain has a complete ordering, i.e. that every physical quantity has a precise determinate magnitude given by a real number (relatively to a unit quantity). Even his definition of the natural numbers did not achieve the generality for which he aimed. He assumed, as virtually everyone else at the time would have done, that the most general application of the natural numbers is to give the cardinality of finite sets. The procedure of counting does not merely establish the cardinality of the set counted: it imposes a particular ordering upon it. It is natural to think this ordering irrelevant, since any two orderings of a finite set will have the same order type; but, if Frege had paid more attention to Cantor's work, he would have understood what it revealed, that the notion of an ordinal number is more fundamental than that of a cardinal number. This is true even in the finite case; after all, when we count the strokes of a clock, we are assigning an ordinal number rather than a cardinal. If Frege had understood this, he would therefore have characterised the natural numbers as finite ordinals rather than as finite cardinals. He was well aware that Cantor was concerned with ordinal rather than cardinal numbers in the first instance;² but, since he never carried his own studies of transfinite arithmetic further than to prove some theorems about Aleph-0 (*'die Anzahl Endlos'*), he dismissed the difference as a mere divergence of interest, and never perceived its significance.

An exception to the rule that philosophers of mathematics pay scant attention to its applications is Wittgenstein. He criticised Frege in this connection,

¹ See Mark Kac and Stanislaw Ulam, *Mathematics and Logic*, Harmondsworth, 1971, p. 161: 'There is little doubt that the "external world" has been the source of many mathematical concepts and theories. But, once conceived, these concepts and theories evolved quite independently of their origins . . . In this evolutionary process, new concepts and theories were generated . . . that, in turn, frequently had miraculous and decisive influence on scientific developments outside of mathematics proper.' On p. 163 they say, 'Then, again miraculously, Hilbert space provided the proper mathematical framework for quantum mechanics'.

² See his review of 1892 of G. Cantor, *Zur Lehre vom Transfiniten*, Halle, 1890.

without, apparently, having understood him, and certainly without appreciating how far more sophisticated Frege's view was than his own. He described Frege's view, maintained against the formalists, as being that 'what must be added to the dead signs in order to make a live proposition is something immaterial, with properties different from all mere signs', and retorted, 'But if we had to name anything which is the life of the sign, we should have to say that it was its *use*'.³ As a critique of a passage in which Frege said that it was applicability alone that raised arithmetic above the rank of a game, this remark is astonishing; but equally astonishing is the crudity of Wittgenstein's conception of the application of mathematics, which would do very well for explaining why 'B-Q6' is not a mere mark on paper. An adequate account of the application of mathematics must, after all, not merely explain how it can be that mathematics is applied, but must do so in a way that does not make it puzzling that there can be such a thing as a pure mathematician. Wittgenstein hankered after a view of mathematical formulas as not expressing propositions, true or false, but as encoding instructions for computation, although he did not attempt to show how such an interpretation could be carried through;⁴ if it could, the existence of pure mathematicians would indeed be hard to explain. Indeed, Wittgenstein's view closely resembles a bad, outmoded method of teaching mathematics in school, which drilled the pupils in techniques of computation without explaining to them why they worked, far less proving that they did or even indicating that such proofs were possible. Frege, by contrast, ascribed to mathematical sentences a sense, which we grasp by apprehending what will determine them as true or as false. The sense, if correctly explained, is intimately connected with the possible applications of the theory to which a given sentence belongs, and so such applications lose their mystery; but it also presents a problem independent of all applications, namely whether the sentence is true or false, and it is therefore likewise unmythical that this problem may be studied for its intrinsic interest, regardless of any extra-mathematical use that may be made of it.

Frege's precept obviously should not be taken as ruling out the theory of a class of algebraic systems defined by their structure, that is, closed under isomorphism, such as groups, rings, Boolean algebras and the like: what encapsulates the general principle of possible applications of any such branch of algebra is a representation theorem. Nor can Frege be read as preaching

³ L. Wittgenstein, *The Blue and Brown Books*, Oxford, 1958, p. 4.

⁴ In his *Remarks on the Foundations of Mathematics*, Wittgenstein asks, 'Might we not do arithmetic without having the idea of uttering arithmetical *propositions*, and without ever having been struck by the similarity between a multiplication and a proposition?', and comments that 'it is a matter of a very superficial relationship' (original edn., ed. G.H. von Wright, R. Rhees and G.E.M. Anscombe, 1956, part I, app. I, § 4, revised edn., 1978, part I, app. III, § 4). Elsewhere he says, 'People can be imagined to have an applied mathematics without any pure mathematics. They can ... calculate the path described by certain moving bodies and predict their place at a given time ... The idea of a proposition of pure mathematics may be quite foreign to them' (original edn., part III, § 15, revised edn., part IV, § 15).

that all applications ought to be foreseen in advance. He knew very well that, in mathematics as opposed to architecture, the construction of the foundations occurred at a late stage in the development of a theory: it is the culmination of the process of rendering it fully rigorous. What, on his view, demands acknowledgement is that an analysis of the general form of the applications of a theory is the proper business of mathematics; no other science is competent to undertake it, so that, while it remains undone, the mathematical theory has not yet been supplied with adequate foundations. If, then, we hit upon an application of a type not provided for in the existing foundations of the relevant theory, we need to analyse what made that application possible, and in the light of that revise the foundational part of our theory, or prove a more general representation theorem, accordingly.

There is an unfortunate ambiguity in the standard use of the word 'structure', which is often applied to an algebraic or relational system – a set with certain operations or relations defined on it, perhaps with some designated elements; that is to say, to a model considered independently of any theory which it satisfies. This terminology hinders a more abstract use of the word 'structure': if, instead, we use 'system' for the foregoing purpose, we may speak of two systems as having an identical structure, in this more abstract sense, just in case they are isomorphic. The dictum that mathematics is the study of structures is ambiguous between these two senses of 'structure'. If it is meant in the less abstract sense, the dictum is hardly disputable, since any model of a mathematical theory will be a structure in this sense. It is probably usually intended in accordance with the more abstract sense of 'structure'; in this case, it expresses a philosophical doctrine that may be labelled 'structuralism'.

Even so, the term 'structuralism' still admits a stronger and a weaker interpretation, comparable to the two interpretations of the phrase 'formal theory' proposed by Frege in his lecture 'Über formale Theorien der Arithmetik' of 1885. On the stronger interpretation, structuralism is the doctrine that mathematics in general is solely concerned with structures in the abstract sense, that is, with systems left no further specified than as exemplifying the structure in question. This doctrine has, again, two versions. According to the more mystical of these, mathematics relates to *abstract structures*, distinguished by the fact that their elements have no non-structural properties. The abstract four-element Boolean algebra is, on this view, a specific system, with specific elements; but, for example, the zero of the algebra has no other properties than those which follow from its being the zero of that Boolean algebra – it is not a set, or a number, or anything else whose nature is extrinsic to that algebra. This may be regarded as Dedekind's version of structuralism: for him, the natural numbers are specific objects; but they are objects that have no properties save those that derive from their position in 'the' abstract simply infinite system (sequence of order-type ω).

That there can be abstract objects possessing none but structural properties is precisely what is denied by Paul Benacerraf: the denial is his ground for holding that numbers cannot be objects. His is therefore the more hardheaded version of structuralism, one misattributed by Russell to Dedekind himself. According to it, a mathematical theory, even if it be number theory or analysis which we ordinarily take as intended to characterise *one* particular mathematical system, can never properly be so understood: it always concerns all systems with a given structure. The difference between, say, number theory and group theory, on this view, is merely that the structure with which the former is concerned is specific; that is, its subject-matter consists of a class of systems isomorphic to one another. It is part of such a view that the elements of the systems with which a mathematical theory is concerned are not themselves mathematical objects, but, in a broad sense, empirical ones; it is not the concern of mathematics whether such systems do or do not exist.

One of the weaknesses of the hardheaded version of structuralism is that, while it may not be for mathematics to say whether or not there exist any systems exemplifying the structures that it studies, the subject would appear futile unless there was a strong chance that they would exist. The more mystical version might seem to escape this difficulty, holding as it does that the purely abstract systems are free creations of the human mind. For Dedekind, however, the process of creation involved the operation of psychological abstraction, which needed a non-abstract system from which to begin; so it was for him a necessity, for the foundation of the mathematical theory, that there be such systems. That was why he included in his foundation for arithmetic a proof of the existence of a simply infinite system, which had, of necessity, to be a non-mathematical one.

Dedekind thus shared with the hardheaded structuralist the need to maintain that we can find infinite systems of objects – systems isomorphic to the natural numbers and others isomorphic to the real numbers – in nature; and the thesis is questionable. It may be held, indeed, that time, for instance, has the structure of the continuum; but this seems more a matter of our imposing a mathematical structure on nature than of discovering it in nature. In his late essay ‘Erkenntnisquellen’, Frege made as robust a declaration as did Hilbert, at just the same time, in ‘Über das Unendliche’, that the infinite could never be found in empirically given reality; but he did not manifest the same conviction in Part III of *Grundgesetze*, which rests on the assumption that there are domains of physical quantities isomorphic to the real numbers.

However this may be, the two types of structuralism – the mystical and the hardheaded – are variants of the strong interpretation of the view that mathematics is about structure. Frege rejected this strong interpretation of structuralism, primarily because it conflicted with his concern for applications: the general type of application to be made of a system such as the natural, the real or, presumably, the complex numbers was, for him, constitutive of those

systems and ought therefore to enter into any correct definition of them. He was, however, himself a structuralist on a weaker interpretation of the term. Mathematics must preserve its logical virginity intact; and hence no concept belonging to physics or any other of the special sciences must ever sully the purity of a mathematical theory, whether in its foundations or its superstructure. For that reason, mathematics should have nothing overtly to do with the details of any specific application of any of its theories or concepts. Hence the principle governing the applications of a mathematical theory, which is to be incorporated into its foundations, must be formulated in completely general terms: it relates to the *structure* of those applications, and in no way to their specific contents.

It is this generality that must be respected both by those who think, like Frege, that the claim of mathematics to be a science derives from its applications, and that the true meanings of its propositions relate to them, and by those who think it is *justified* by those applications. Because the radical formalists make application external to mathematics, each application has to be treated separately, consisting as it does, for them, in devising a particular empirical interpretation of a formal calculus; there can therefore be no general principle. The same holds good for a neo-Hilbertian like Hartry Field. From the standpoint of a highly selective nominalism, which abhors real numbers, but countenances space-time points, and even sets of them, as being sufficiently physical entities, he rejects all claims that such a theory as real analysis could actually be *true*. On his view, it must, rather, be justified indirectly, as possessing a stronger property than that of being consistent with a scientific theory within which it is applied, namely that of yielding a conservative extension of that theory when adjoined to it. We have thus to show that anything statable in terms of the scientific theory and provable from the composite theory could have been proved from the scientific theory alone.

The notion of a conservative extension makes sense only if the theory to be extended is formulated in a language more restricted than that of the extended theory. Hence, to give sense to Field's claim, he has to make the prior claim to be able to reformulate scientific theories so as to avoid any apparent reference to the spurious objects of the mathematical theory such as real numbers; indeed, if the nominalistic motivation is to be satisfied, reference to all other abstract objects unacceptable to a nominalist of his persuasion must also be eliminated. This reformulation is the harder of Field's two tasks: how can he so frame physical theories as to eschew all abstract objects?

These difficulties would vanish for anyone convinced of the soundness of Frege's invocation of the context principle to yield a *general* justification of abstract (non-actual) objects. The existence of mathematical objects presents especial problems, however, as Frege was already aware when he wrote *Grundlagen*, became more vividly aware in writing *Grundgesetze*, and was made pain-

fully more aware yet by Russell's contradiction. Hence, even for someone free of qualms about abstract objects in general, Field's objective retains an interest as a means of indirectly justifying appeal to specifically *mathematical* objects and structures.

Field envisages the indirect justifications at which he is aiming as being obtained piecemeal; but this violates Frege's principle of generality, that a uniform explanation be provided for all the applications that may be made of any given mathematical theory. Consistency is an absolute property of an arithmetical theory; so is analyticity: such a theory has the one property or the other irrespective of whatever other theory, physical or mathematical, it may be applied to. Frege argued consistency to be too weak a property to warrant our concluding to the truth of propositions derived by applying the theory, and claimed analyticity as necessary for this. Conservativeness, on the other hand, is not merely an intermediate property: it is relative to the theory within which the application is made. Hence success in one case would not guarantee success in another: it therefore appears that the programme would have to be carried out separately for each scientific theory in which the mathematical theory found application. Even if we accomplished the task for all existing scientific theories, it would have to be done afresh for any new theory that was devised that made use of our mathematical theory. But this is contrary to reason. The theory of functions of a real variable, for example, is surely not one that requires separate justification for each application that is made of it: to whatever extent it needs justification, it must be justifiable once for all, in such a way as to be available both for the formulation of a scientific theory and for use in conjunction with it.

Suppose that some institute undertook to try to carry out Field's programme, vis-à-vis the theory of real numbers, for all known scientific theories: and suppose that it achieved definite results in every case. If it established that, for one or more scientific theories, the programme could *not* be carried out, we should have to conclude that the theory of real numbers required a justification of a kind different from that envisaged by Field. If, on the other hand, it was shown that the theory of real numbers yielded a conservative extension when added to any one among all known scientific theories, we should surely suspect that some general principle was involved, and that we were wasting our time tackling each scientific theory individually. Indeed, the repeated success of the programme would *demand* a general explanation. Presupposing the feasibility of his plan of reformulating physical theories, Field argues that those who consider mathematical theories to be true necessarily or a priori must allow that they have the weaker property that, added to any other theory whatever, they will yield a conservative extension of it; the fact that so many have held them to have the stronger property is, he thinks, suasive evidence that they have the weaker one, at least relatively to reformulated physical

theories.⁵ But this argument cuts both ways. If it can be shown that some mathematical theory always yields a conservative extension when added to a scientific theory, this fact stands in need of some uniform explanation: what better explanation could there be than that the theorems of the mathematical theory are analytically true? No doubt, in view of the difficulties mathematical objects pose for logicism, this explanation is not available; but it is better to search for some related property possessed by mathematical theorems than to acquiesce in a case-by-case justification of applications of them. Of this we may be quite certain, for any given mathematical theory: either Field's programme cannot always be carried out; or it can, and there is some general explanation of that fact which will, of itself, constitute a justification of that theory.

If Field's general objection to abstract objects were replaced by a restricted objection to mathematical ones, would anything then remain of Field's strategy for avoiding reference to such objects? That strategy would be considerably simplified, but the programme would not become altogether otiose. Field's strategy is to reformulate a given scientific theory in nominalistic terms, and then to prove a representation theorem for the reformulated theory in terms of real numbers (or of whatever objectionable mathematical objects are appealed to in the usual formulation). The nominalistic reformulation replaces references to such quantities as temperatures by predicates applying to space-time points; for instance, one stating intuitively that the temperature at y is intermediate between that at x and that at z , one stating intuitively that the difference between the temperatures at x and y is equal to that between those at z and w , and one stating intuitively that the temperature at x is less than that at y . (To deal with mass in this way, we need to consider density at a point.)⁶ With the general objection to abstract entities waived, there is no reason why the reference to quantities that occurs in the usual formulation should be eliminated. Those quantities would not be postulated to be represented by real numbers, however, or by numbers of any other kind; the properties of the quantities treated of in the theory that result from their numerical representation would have to be stated directly, so as to allow of the subsequent proof of a representation theorem. In this way, the scientific theory would still require reformulation.

The non-nominalistic modification of Field's programme thus sketched provides a glimpse of how the generality principle might be reinstated. If Frege's characterisation of the real numbers as ratios of quantities is accepted, then, given a far better analysis than he provided of when properties assigned by a physical theory to bodies (or to space-time regions or points) are to be

⁵ H. Field, *Science Without Numbers*, Oxford, 1980, pp. 12–13.

⁶ To treat mass density as primitive certainly violates the requirement of respect for conceptual priority, since we normally think of density as mass/volume, rather than of mass as the product of density and volume. It may be retorted that the requirement is exorbitant when imposed on formulations of physical theories.

classified as quantities, a general representation theorem could be proved for theories satisfying those conditions; such an analysis is supplied by measurement theory. We should then have an account of the application of real numbers that diverged far less widely from that at which Frege aimed, but fell short of supplying, than that proposed by Field himself.

It would not even be necessary, within such a modified version of Field's programme, to eliminate all reference to real numbers within the physical theory itself, since they could be treated as ratios between the quantities of which the theory treated, rather than as given antecedently by the mathematical theory. Such real numbers would not be *mathematical* objects, for the distinguishing characteristic of mathematical objects is that their existence presupposes nothing about empirical reality; these real numbers would, rather, be dependent abstract objects in the same sense as that in which the Equator depends for its existence upon contingent features of the world: which real numbers existed would depend upon which quantities existed, according to the theory. If we call the Equator an abstract geographical object, real numbers so explained could be called abstract physical objects. For that reason, their existence would be unproblematic in the light of the context principle. What would now be violated would be the purity of the mathematical theory; for the theory of these real numbers would no longer be a branch of pure mathematics, but a fragment of the physical theory.

All this leaves the problem of mathematical objects unresolved; but, if we set that problem aside, we can surely say that Frege's ideas concerning the application of mathematics were surely sound in outline. It cannot be by a series of miracles that mathematics has such manifold applications; an impression of a miraculous occurrence must betray a misunderstanding of the content of the theory that finds application. Frege was right to hold that it belongs to the task of mathematics to analyse the principles in accordance with which each mathematical theory is capable of being applied, not separately for each application, but in a general fashion that will cover them all. The genesis of most mathematical theories was due in the first instance to the need to arrive at a logical analysis of one or another empirical problem. Certainly the development of the theory requires us to 'leave the ground of intuition behind': we do not have a properly *mathematical* theory until we have ceased to rely upon our apprehension of the perceptible or experiential, and have attained that generality which Frege would think entitled the theory to be recognised as a branch of logic – the generality demanded by structuralism in its weaker sense. At this stage, the theory is likely to admit a wider class of applications than those which originally prompted its development; but, when we have reached the stage of setting the theory upon firm foundations, we must not be tempted by the strong version of structuralism to lose sight both of the original applications and of possible future ones. The historical genesis of the theory will furnish an indispensable clue to formulating that general principle governing all possible

applications of it which Frege demanded should be incorporated into its foundations. Only by following this methodological precept can applications of the theory be prevented from assuming the guise of the miraculous; only so can philosophers of mathematics, and indeed students of the subject, apprehend the real content of the theory. Admittedly, the prescription that the general principles governing the application of the objects of a mathematical theory should be incorporated into their definition is difficult, perhaps impossible, always to follow in practice. It remains clear that, whenever practicable, it is the most direct way of embodying in the foundations of the theory an analysis of what renders its applications possible; but the most important thesis advanced by Frege in this connection is that such an analysis must be so embodied by some means, as being the proper business of mathematics, and that which renders it a science – a genuine sector in the quest for truth. Frege failed, by quite a large margin, to achieve the analysis that his philosophical principles demanded, even in the two comparatively elementary cases that he tackled: but he surely pointed the direction we need to take.

Platonism and logicism

Platonism is the doctrine that mathematical theories relate to systems of abstract objects, existing independently of us, and that the statements of those theories are determinately true or false independently of our knowledge. This doctrine has an obvious appeal to the pure mathematician, but raises immediate philosophical problems. How can we know anything about this realm of immaterial objects? And how can facts about it have any relevance to the physical universe we inhabit – how, in other words, could a mathematical theory, so understood, be *applied*? Logicism is not a natural ally of platonism, because, on the most natural view of logic, there are no logical objects: it was a tour de force on Frege's part to combine a vehement advocacy of platonism with an unreserved logicism about number theory and analysis. The most celebrated later advocate of platonism, Kurt Gödel, presented it in a non-logicist form, in which, indeed, it prompts the two objections concerning the applications of mathematics and our knowledge of it. To the former he had, so far as I know, no very good solution; the latter he solved by postulating a faculty of intuition of abstract objects, in analogy with the perception of material ones. But this raises the further problem, why proof is so salient in mathematics. The search for new axioms for set theory, recommended by Gödel, might be compared to the observations made by astronomers; but, if the analogy with the physical realm were sound, what would explain why mathematicians spend so little time on such observations, and so much on eliciting by means of complex deductions the consequences of facts already observed? Why, indeed, do they not elaborate speculative theories which need testing by further observations, as natural

scientists do, and why do they demand incontestable *proof*, rather than high probability, as a warrant for asserting a mathematical proposition? Uncertainties about the formation of stars, or the behaviour of Cepheid variables, do not reflect any haziness in our grasp of the concept of a star, but only a defect in our knowledge of the behaviour of stars. Likewise, if the analogy between physical and ideal objects were sound, our uncertainty about the continuum hypothesis need show no haziness in our *concept* of a set, but only in our knowledge of what sets God has chosen to create; for presumably ideal objects are as much God's creation as physical ones. Physical objects have many properties neither revealed by immediate observation, nor deducible from those so revealed; we can hope to discover them only by making further observations, and, from a realist standpoint, cannot be certain of discovering all of them even then. Were the analogy sound, mathematicians would treat the ideal objects which they study in a similar way: the inappropriateness of such a description of their activities serves to point the lameness of that analogy.

Frege, in virtue of his logicism, had none of these objections to face. If the natural view is taken of logic, according to which there are no logical objects, the logicist programme, if it could be carried out, would provide an interpretation of all mathematical statements in the language of higher-order logic. This interpretation would dispense with all mathematical objects, which would disappear in favour of higher-order properties and relations; we should thus have a non-platonist logicism. The application of mathematical statements would then be quite unproblematic: application would simply consist in instantiation of universally quantified formulas. This was, in effect, what Whitehead and Russell attempted in *Principia Mathematica*, since their classes are only surrogate objects, affording a disguised means of speaking of (higher-order) properties and relations; the ramified hierarchy of types seeks to evade the objections to the impredicative character of higher-order quantification. Their attempt ran against the difficulty that would have supplied the only valid ground for Frege's insistence that numbers *are* genuine objects, the impotence of logic (at least as they understood it) to guarantee that there are sufficiently many surrogate objects for the purposes of mathematics, forcing them to make assumptions far from being logically true, and probably not true at all: to secure the infinity of the natural-number sequence, they had to assume their axiom of infinity, and to secure the completeness of the system of real numbers, they had to assume the axiom of reducibility.

Frege failed to establish the logicist thesis, as he himself understood it; but, when we declare that he failed to establish it, we are inclined to forget that his interpretation of it was more generous than ours, just because he believed in logical objects, and we, taking a narrower view of what logic is, do not. On his definition, a statement is analytically true if it can be derived, by the help of definitions, from a logical truth; a broader or a narrower conception of analyticity must then result from adopting a broader or a narrower conception

of logic. Frege's attempt to prove the analyticity of arithmetical truths made it even easier than Russell's did to see where it failed, when we construe analyticity in accordance with our narrower conception of logical truth. Frege needed to appeal to the status of natural numbers as objects solely in order in order to prove the infinity of the natural-number system; we may conclude that he succeeded in showing to be uncontroversially analytic all arithmetical propositions that do not require the existence of infinitely many natural numbers – essentially, finitistic statements in Hilbert's sense. A proposition may be said to be uncontroversially analytic if it is analytic on the narrower conception, according to which there are no logical objects: the thesis that all arithmetical truths are uncontroversially analytic would be that of non-platonist logicism.

Non-platonist logicism was not a possible route for Frege because it allows no access to the infinite totalities he took to be essential for mathematics. It is not merely that he would have been unable to *prove* that there are infinitely many natural numbers – it could after all be objected that his alleged proof is circular: it is that we should have no reason to suppose it *true* that there are infinitely many natural numbers. That is why his combination of logicism with platonism, had it worked, would have afforded so brilliant a solution of the problems of the philosophy of mathematics. The logicism explained how mathematics could be applied, how we could know mathematical propositions to be true, and whence their necessity derived; the platonism justified the existence of mathematical objects and clarified their status. Frege's idea was that such objects should always be defined as extensions of concepts directly related to the application of the mathematical theory concerned: concepts to do with cardinality in the case of the natural numbers (and other cardinal numbers), concepts concerning the ratio of one quantity to another in the case of the real numbers. In this way, application could be understood as being no more problematic than it would be according to non-platonist logicism: it would not consist in pure instantiation of formulas of higher-order logic, but would involve deductive operations so close to that as to dispel all mystery about how application was possible. A mathematical theory, on this view, does indeed relate to a system of abstract objects existing independently of us. They are not, however, pure abstract objects in the sense in which we speak of pure sets (sets all the members of whose transitive closures are also sets): they are objects characterised in such a way as to have a direct connection with non-logical concepts relating to any one of the particular domains of reality, the physical universe among them. They could not otherwise have the applications that they do.⁷

⁷ Thus Frege's cardinal numbers must be thought of as containing classes whose members are actual objects. This indeed conflicts with his implication in *Grundgesetze*, vol. I, § 10, that the objects of the theory are restricted to truth-values and value-ranges. But this implication, though not inadvertent in the context, makes nonsense of his plain intention that his cardinal numbers will be those involved in empirical ascriptions of number; a similar remark applies to his real numbers.

One reason why it is convenient to express mathematical theories in terms of objects such as numbers of various kinds is that non-logical abstract objects frequently figure in the physical theories to which the mathematical ones are applied. The cause of nominalism cannot be advanced by dispensing with mathematical objects such as real and complex numbers, but allowing point-instants in physical space-time to continue to infest the physical theory; if abstract objects do not deserve entry visas, they cannot improve their case by producing passports issued by physics rather than mathematics. The converse, of course, does not hold: if the context principle licenses reference to abstract objects in general, that does not imply a liberty to assume the existence of mathematical objects of all kinds. Why, then, does there appear to be a compelling need for mathematical objects? The need arises from the concern of mathematics with infinity. It has to be concerned with infinity because of the generality of its applications: even if we were fully convinced that everything to which mathematics would ever be applied would be thoroughly finite, we cannot set an upper bound in advance on the number of its elements, or a lower bound on the ratio of its magnitudes. There cannot be infinitely many properties or functions unless there are infinitely many objects to start with; infinity must be injected at the lowest level. Granted, for a particular application, the mathematical theory might borrow its objects from physics, or whatever other empirical science it was being applied to, if that science claimed an infinity of them to dispose of. This, however, would both violate the purity of the mathematical theory, and offend Frege's principle of generality: as he insisted, that theory is not of itself concerned with particular applications, but with the general principle underlying them. It must therefore be justified once for all, and not separately for each application. This requires that it have its own objects, and not borrow them from different physical theories in turn. Frege argued, correctly, that the bare consistency of a theory does not suffice to warrant its applications, within mathematics or outside it; he concluded that, to justify such applications, we must require the theory to be true. He admittedly did not envisage the possibility canvassed by Field of showing it to have a property stronger than consistency, but weaker than (analytic) truth; we may leave it to the final chapter to consider whether this is a genuine alternative.

It is not inaccurate to express this by saying that mathematics must be adapted for all possibilities. A less happy formulation is that it is concerned with possible, not really existing, objects; and this has suggested a formulation of mathematical theories using modal logic. The suggestion does not, however, go to resolve any genuine dilemma. The problem which Frege failed to solve was to specify definite truth-conditions for statements involving reference to and quantification over value-ranges, which required a determination of what value-ranges were to belong to the domain; our problem is to do the same for, say, real numbers. If we follow Frege in deriving the existence of real numbers from that of infinite sets of natural numbers, the problem reduces

to that of specifying truth-conditions for statements involving quantification over such sets (which we shall be unable to do even in modalised mathematics if we have failed to provide for the possibility of there being infinitely many natural numbers). If we do not follow Frege in this regard, the problem becomes that of specifying truth-conditions outright for statements about real numbers. It does not help to rephrase the problem as that of specifying truth-conditions for statements about what real numbers there might be; it remains essentially the same. The flaw in Frege's philosophy of arithmetic – the flaw that caused it to crack apart – was his erroneous justification of the existence of logical, and hence of mathematical, objects. Doubtless this problem is to be solved piecemeal, in a different way for different cases, rather than simultaneously for all possible cases, as Frege hoped; his failure to solve it should not be allowed to obscure all other aspects of his philosophy of arithmetic.

The fruitfulness of deductive reasoning

Deductive inference patently plays a salient part in mathematics. The correct observation that the discovery of a theorem does not usually proceed in accordance with the strict rules of deduction has no force: a proof has to be set out in sufficient detail to convince readers, and, indeed, its author, of its full deductive cogency. The philosophy of mathematics is concerned with the *product* of mathematical thought; the study of the process of production is the concern of psychology, not of philosophy. Although a theory – number theory, for example – may thrive for a long time before anyone thinks to axiomatise it, experience suggests that all mathematical theories, when sufficiently developed, are capable of axiomatisation, though often only in an essentially second-order language. The failure of the logicist thesis can therefore be localised in the justification of the axioms; a great part, at least, of the necessity of mathematical theorems is the necessity of deductive consequence. Mill and Frege are virtually the only two philosophers to have addressed what is surely the most striking, and perplexing, fact about deductive reasoning, namely its capacity for yielding new and often surprising knowledge; the difficulty is to explain this capacity without undermining our perception of its cogency. For it to be cogent, we must be allowed to be able to recognise that whatever renders the premisses of a deductive step true already renders its conclusion true; for it to be fruitful, we must be able to grasp the premisses and acknowledge them as true without perceiving the possibility of drawing that conclusion.

Frege's solution of this problem must be along the right general lines. If deductive inference were not a creative process, proving theorems would be a mechanical activity; Frege sought to explain its creative character as involving the recognition of patterns common to different thoughts – patterns there to be recognised, whose recognition was nevertheless not required for the

thoughts to be grasped. He was satisfied to restrict the recognition of such patterns to a particular type of case, namely the discernment of complex first- or higher-order predicates within a sentence or of functional expressions within a complex term; though undoubtedly important, this special case is surely inadequate to bear the full weight of an explanation of the fruitfulness of deductive reasoning. The discernment of common patterns that accounts for its fruitfulness is not to be confined to patterns exhibited by individual propositions, but must relate also to sequences of propositions that make up a proof or the description of an effective procedure. The proof or procedure usually does not require a unique ordering of the propositions, which may be rearranged without destroying the validity of the proof or the effectiveness of the procedure; perception of a pattern common to two such sequences will normally require apprehension of the possibility of such a rearrangement. This topic deserves detailed study by cases, which it has not received because philosophers seem oddly uninterested in it, being content to accept that deductive reasoning *is* both cogent and astonishingly fruitful without bothering their heads to explain how this can be so. There can be little doubt, however, that the general idea underlying Frege's explanation of its fruitfulness must be correct; it is difficult to see how an explanation could be offered along any other lines.

The Problem of Mathematical Objects

The necessary existence of mathematical objects

The logicist thesis failed because of its inability to justify the existence of mathematical objects, more particularly of systems of objects satisfying the axioms of the theories of natural numbers and of real numbers. More precisely, Frege's attempt to establish the thesis failed, even according to his more generous interpretation of it, because his application of the context principle failed to justify their existence. The problem is best thought about in connection with the necessary truth of mathematical statements. Their necessity is enough to rule out our possession of a faculty of mathematical intuition conceived in analogy with perception: if this were the source of our mathematical knowledge, the propositions of mathematics would be as contingent as those of astronomy. The existence of a system of mathematical objects is like the existence of God in this, that one may believe in it or disbelieve in it, but one cannot intelligibly say that it exists but might not have done, or does not exist but might have done. It differs from the existence of God in that God, according to the theologians, is the most actual of all beings, whereas mathematical objects are non-actual. Hence, while the incoherence of the conception of God would show that he does not exist, the mere coherence of the conception is not enough to show that he does. The necessity of God's existence derives, rather, from its being the condition for the existence of everything else, so that there is no prior condition of which we can say that, if it had not been fulfilled, God would not have existed. That is why, as Aquinas perceived, the necessity of God's existence does not entail that we can know it a priori. All that we know a priori is that, if God exists, then he exists necessarily.

By contrast, if we are able to know that a system of mathematical objects exists, we can only know it a priori: it makes no sense to suppose that we might know it by some a posteriori means. It must therefore be from the possibility of our knowing its existence a priori that the necessity of its existence derives; and this entails that the coherence of the conception of the system is

sufficient, in the light of the context principle, to justify the assertion of its existence.

It is on this, and not on the contention that the notion of class belongs to logic, conceived as the science of deductive inference, that Frege's claim that arithmetical truths are analytic ultimately rests. By no means all abstract objects exist of necessity: the Equator does not, for one. Mathematical objects, when genuine, do, because the truth-conditions for statements about them have been fixed in such a way that no condition for their existence needs to be fulfilled; that is why Frege felt entitled to call them logical objects. He did attempt to make clear his criterion for applying the epithet 'logical', namely to what governs every realm of reality and every degree of reality – the merely thinkable as well as what in fact exists. He was doubtless at fault, however, for failing to make clear what, in general, he conceived as belonging among the fundamental logical laws. He indeed claimed Axiom V of *Grundgesetze* as being among those fundamental laws, but we, accustomed to think of the laws of logic as restricted to those governing deductive inference, misunderstand his ground for doing so; we are not helped to understand him aright by the fact that this particular 'law' is self-contradictory, or by his viciously circular attempt to justify it.

The nature of mathematics

Frege's conception of what belongs to logic was indeed more generous than that which is natural to us; but the foregoing way of drawing the contrast, between universal applicability and relevance to deductive inference, distorts his view. It is not that logical objects were for him irrelevant to deductive inference; it was merely that he did not expect their relevance to it to be apparent outside mathematics. For him, the whole point of mathematics lay in its applications. A mathematical theorem, on his view, encapsulates an entire deductive subroutine – perhaps a very complex one – which, once discovered, does not need to be gone through again explicitly on future occasions; but it expresses it, not as a principle of inference, but as a proposition to which we have given sense by fixing its truth-conditions, and which may therefore be considered on its own account, without an eye on its possible applications. On this view, therefore, that part of mathematics which is independent of intuition simply comprises all the complex deductive reasoning of which we are capable, purged of all that would restrict its application to particular realms of reality. (We might qualify this as all such reasoning as involves only completely definite concepts; Frege himself believed genuine reasoning with imperfectly definite concepts to be impossible.) Geometry apart, mathematics therefore simply *is* logic: no distinction in principle can be drawn. Most of the deductive reasoning which it in this way encapsulates requires, for its formulation, reference to abstract objects – mathematical or logical objects; we might use this as a

criterion, not for demarcating mathematics from logic, but for singling out the mathematical part of logic, since the far less complex kinds of deductive reasoning that do not require mathematical expression need no reference to logical objects. It is difficult to maintain that any more convincing account of the general nature of mathematics has ever been given.

It is apparent from this account how misguided it is to criticise Frege for reducing one mathematical theory, arithmetic, to another, set theory. He would have had no objection to considering the notion of class as a mathematical one, but would not have seen that as in any way conflicting with characterising it as a logical one. His reasons for regarding it as a logical notion, namely that a class cannot be considered as a whole made up out of its members, but must be explained as the extension of a concept, were indeed sound: given his assumption that every concept has an extension (and every function a value-range), they were cogent. His initial attempt at avoiding Russell's contradiction retained this assumption (allowing the abstraction operator still to be applied to any expression for a first-level function of one argument), resorting to the desperate expedient of denying that, to have the same extension, concepts (and functions) needed to be co-extensive. When he discovered the inadequacy of this solution, he rejected the notion of a class (of the extension of a concept) as altogether spurious; had he taken a less hostile view of it, he would still presumably have denied it to be logical in character, in having proved to lack the required generality. For the failure of his solution indicated the impossibility of retaining the assumption that every concept has an extension; with this assumption gone, it looks unlikely that all answers to the question, 'How many objects fall under the concept F ?', can be explained in terms of the extension of the concept F . The natural assumption, which Frege is very likely to have made, is that that question has an answer whenever the concept F is definite and (unlike the concept *red*) is defined over a determinate domain or has a criterion of identity associated with it. On this assumption, the notion of cardinality has sufficient generality to be recognised as logical in character; Russell's paradox had shown, contrary to first impression, that that of the extension of a concept does not. It may indeed be replied that it is only when the concept F has an extension (determines a set) that the question, 'How many objects fall under it?', has an answer, so that the two notions have the same generality; but, even after he had recognised the inadequacy of his solution of the contradiction, Frege is unlikely to have attained that paradoxical conclusion (which, indeed, is rejected when it is said that a class is proper when it has the same cardinality as the universe). It is not, of course, that Frege did not make a grave mistake: only that to characterise the mistake as that of reducing a simpler mathematical theory to a more complex one is to misconceive both his objective and the distance by which he fell short of attaining it.

The important claim Frege made is that there exists a method of characteris-

ing a system of mathematical objects which serves to confer senses upon the statements of that mathematical theory of which the system is a model in the light of which the context principle guarantees that we do make genuine reference to those objects. The existence of that system is therefore a priori and independent of intuition, and the axioms of the theory may rank, accordingly, as analytic. Frege believed that he could, by introducing value-ranges, thereby introduce all logical objects that would be required in mathematics; and he had a quite erroneous idea of how to give a coherent and presuppositionless characterisation of the system of such value-ranges. These mistakes do not invalidate the general claim; if it can be sustained, we have a highly plausible account of the character of mathematics in general.

The existence of mathematical objects

The conception of mathematical intuition as analogous to sense-perception is open to an evident objection. A physical complex apprehended by the senses may prove to have properties not immediately apparent, just as a mathematical system may prove to have properties not apparent from our initial grasp of it. But, whereas those of the physical system need in no way be implicit in our means of identifying it, those of the mathematical system must be; this would not be true if mathematical intuition were analogous to sense-perception. If the continuum hypothesis, say, is determinately true, that can only be because it follows from principles not yet formulated by us, but *already* inchoately present in our intuitive conception of the intended model of set theory. If that conception were a kind of blurred perception, on the other hand, it might be that it could be filled out, with equal faithfulness to our present grasp of it, however implicit, both so as to verify and to falsify the continuum hypothesis, which nevertheless possessed a determinate truth-value according to the way things happened in fact to be. Since this supposition is manifestly absurd, this path to justifying the existence of mathematical objects, without appeal to the context principle, is closed.

Can Frege's thesis that it is possible to justify a priori the existence of a system of mathematical objects be sustained in the face of his own failure to produce an acceptable vindication of it? The thesis amounts to a claim that the fact that a given conception of a system of mathematical objects is coherent is enough to warrant asserting the existence of that system; that it is in effect self-justifying. This is not intended as an admission that mathematical existence is after all to be equated with consistency. The theory of negative types (derived from the theory of simple types by allowing negative and positive integers to serve as type-indices) is obviously consistent if the theory of simple types is, since any proof can be reinterpreted in the latter theory; but that fact does not of itself suffice to justify our believing in the existence of a system of sets so stratified. In the intended sense, the coherence of a conception of

a system of objects demands much more than its merely not involving a contradiction. It requires that we should have a clear grasp of the range of individual objects that the system comprises, and of the constitutive relations between them, enabling us to recognise the truth of fundamental axioms governing the mathematical theory which describes that system. For the claim that a coherent conception of the system suffices to ensure its existence is based upon the context principle, applied not to a circular procedure like Frege's own, but to some legitimate means of fixing the senses of statements concerning it.

The possession of such a conception of a mathematical system – of an intuitive model for the theory that relates to it – is without question essential for us to have a mathematical theory at all, rather than a mere piece of formalism; and it is this which tempts us to speak of mathematical intuition. The term would not be altogether inappropriate, were it not hard to resist the pressure to interpret it as denoting something analogous to sense-perception; that pressure makes it, too, a dangerous piece of terminology. The danger lies in its creating the impression that the grasp of an intuitive model for a theory is unmediated by language: that we perceive its structure by a direct intellectual apprehension. If it were 'so, it would be useless, because it could not be conveyed to others: only a solipsistic mathematics could result from alleged intuitions of this kind. In fact, we have no such powers: we frame intuitive models by means of concepts common to us all, and the models have no more content, and are no more definite, than the verbal or symbolic descriptions by means of which they may be communicated.

But can an intuitive conception of a mathematical system be sufficiently sharp as to be self-justifying, so that the mere possession of that conception warrants the assertion that such a system exists? It is a belief that it can that leads to the talk, so dear to Dedekind, of mathematical objects as the free creations of the human mind; but there is no such thing as *the* human mind, only individual minds. The metaphor is dangerously psychologistic, tempting us to scrutinise the internal operations of our minds. A conception of a mathematical system – an intuitive model – cannot transcend the means – necessarily linguistic and symbolic means – by which one person can convey it to another; it exists only in so far as it can be described. Frege would insist that a system so conceived existed independently of being conceived. Saying that has its danger, too – that of suggesting that something more is needed for its existence than our having a clear conception of it: it is only a step from that to thinking of mathematical reality as contingent – a matter of which constituents of it God has chosen to endow with existence. It would certainly be wrong to say that the system existed *in advance* of our conceiving it, because it does not exist in time at all; but if we say that we created it, we have to regard it as having come into existence, and as not having existed previously.

A non-Fregean answer

The fact remains that it is extremely difficult to frame a clear description of a mathematical system, as intuitively conceived, at least when it is fundamental in being the source of general notions that we use in many other contexts, and particularly when it is from them that our notion of a particular infinite cardinality is derived. Attempts to do so, at least for systems of cardinality greater than the natural numbers, always have a certain cloudiness, and leave some quite unconvinced that any sharp conception is being conveyed, while satisfying others. Furthermore, it is notoriously hard to resolve such disagreements over whether or not a given conception of such a system is so much as coherent, let alone sharp, or even to see by what means they could be resolved.

Why is this? Frege can give us no help at this point: in so much as discussing the matter, we have had to leave him behind. We are trying to solve the problem he failed to solve, in his spirit but in a different way; and the attempt has simply led us into the presence of a range of familiar philosophical disagreements which more resemble differences of taste than divergent rational conclusions. Discovering the correct way out of this impasse is not relevant to an exposition of Frege's work, only to evaluating it.

Logicism, as represented first by Frege and then by Russell and Whitehead, failed because it combined three incompatible aims: to keep mathematics uncontaminated by empirical notions; to represent it as a science, that is, as a body of *truths*, and not a mere auxiliary of other sciences; and to justify classical mathematics in its entirety. There are still those who wish to abandon the first of these three aims, and revert to an empiricist conception of mathematics as a natural science like any other; but Frege's arguments against such a conception were surely conclusive. Field recommends abandoning the second aim: mathematics, for him, is the servant, not the queen, of the sciences, and should refrain from giving itself airs. His strategy for proving the conservativeness of a mathematical theory *S* over a physical theory *T*, formulated nominalistically, is first to prove a theorem that a model of *T* can be constructed in *S*, and then to construct a model of *S* in (an adaptation of) Zermelo-Fraenkel set theory *ZF*. The final step is to prove that, if *ZF* is consistent, so is *ZF* + *T*. Now if *S* is a second-order theory, we need the second-order version of *ZF*, which we must assume to be 'semantically consistent', i.e. to have a model: we obtain conservativeness with respect to model-theoretic consequences. Field hopes, however, that first-order formulations of physical theories will be sufficient for the purposes of physics, and first-order versions of mathematical theories sufficient for applications to physics. In this case, we need consider only the usual first-order version of *ZF*, and shall obtain conservativeness with respect to proof-theoretic consequences, a result which 'follows merely from the consistency of *ZF*',¹ i.e. its consistency in the ordinary

¹ H. Field, *Science without Numbers*, Oxford, 1980, p. 19.

(proof-theoretic) sense. Merely? How does Field know, or why does he believe, ZF to be consistent? Most people do, indeed; but then most people are not nominalists. If ZF is consistent, then, being a first-order theory, it has a denumerable model; but it is not from such a model that Field derives his belief in its consistency, since he has no reason to suppose it to exist save by assuming the consistency of the theory. Our primordial reason for supposing ZF to be consistent lies in our belief that we have an intuitive model for it, the cumulative hierarchy in which the sets of rank $\alpha + 1$ comprise 'all' sets of elements of rank α (together with the elements of rank α , it is necessary to add when we start with Urelemente). The constructible hierarchy yields a more restrained model; but, considered as an intuitive model, it requires that we have a grasp of the totality of ordinal numbers less than the first strongly inaccessible one. Such a model is, from an ordinary standpoint (not that of traffickers in large cardinals), of enormously high cardinality: it is to the field of real numbers as a skyscraper to a two-storey farmhouse. If we have a conception of such a structure, why should we jib at the system of real numbers? If ever there were a case of a pointless reduction of (the conservativeness of) a mathematical theory to (the consistency of) a more complex one, it is to be found here, and not in Frege's work.

Field indeed offers a reason for believing ZF to be consistent, namely that 'if it weren't consistent someone would have probably discovered an inconsistency in it by now'.² He refers to this as inductive knowledge.³ To have an inductive basis for the conviction, however, it is not enough to observe that some theories have been discovered to be inconsistent in a relatively short time; it would be necessary also to know, of some theories not discovered to be inconsistent within around three-quarters of a century, that they *are* consistent. Without non-inductive knowledge of the consistency of some comparable mathematical theories there can be no inductive knowledge of the consistency of any mathematical theory. Since Field claims no non-inductive knowledge of the consistency of any theory, he can have no knowledge of consistency at all.

If the problem of mathematical objects is not to be solved by abandoning either of the first two aims, perhaps we need to abandon the third; and, in particular, the assumption, in which Frege had an unswerving faith, that, given any domain of mathematical objects, quantification over it can be interpreted classically, so that statements formed by means of such quantification will be determinately either true or false, and hence obey classical logic. His faith in this assumption constitutes his sole blindness to the fundamental problems of the philosophy of mathematics: he had at least the excuse that, when he was writing his major works, no one had yet raised the question.

Intuitionists deny the assumption for quantification over any infinite totality,

² H. Field, *Realism, Mathematics and Modality*, Oxford, 1989, p. 232.

³ *Ibid.*, p. 88.

on the ground that it is impossible to complete an infinite process. Independently of any such general doubt, however, the assumption demands that the conception of the domain be completely definite: any haziness about what elements it does or does not contain must obviously vitiate the assumption. Provided that there are some clear general principles concerning the condition for membership of the totality, and some means of identifying individual elements with an indisputable claim to belong to it, both universal and existential quantification over a hazily circumscribed totality can have an intelligible sense, in that they sometimes yield statements recognisably true or recognisably false. Such quantification cannot, however, be construed as invariably yielding statements with determinate truth-values. Statements involving it must be regarded as making claims which their authors make justifiably if they are capable of vindicating them. If the claim embodied in such a statement can be vindicated, the statement may be regarded as true; if the claim is shown impossible to vindicate, the statement may be taken to be false; but, if neither vindication nor refutation is forthcoming, it cannot be presumed to be either.

A realist view of the external world involves assuming that universal or existential generalisation over a totality given by an empirical concept, such as the concept of a star, *does* yield statements determinately true or false, provided that the concept is definite. Truth-conditions differ from conditions for the justifiability of a claim in that they obtain independently of the speaker's knowledge or his capacity to perform some task (unless of course they are the conditions for the truth of some statement about such matters). A concept is definite provided that it has a definite criterion of application – it is determinate what has to hold good of an object for it to fall under the concept – and a definite criterion of identity – it is determinate what is to count as one and the same such object. On a realist view, we do not need, in the empirical case, to be able to circumscribe the extension of the concept more closely in order to be assured that generalisation with respect to it will yield statements with determinate truth-values, independent of the speaker's warrant for making them. We do not need to be able to say just what objects there are which fall under the given concept: provided the concept is definite, reality will of itself determine the truth or falsity of such statements. On this view, *reality* dispels all haziness: we need do nothing further to eliminate it.

Frege was a resolute realist about mathematics, as about the external world; but even he did not argue that mathematical reality will determine the truth-values of mathematical statements, without any need for us to circumscribe the domain of quantification or to specify what objects belonged to it. He did not *argue* in this way: but he may be accused of having in effect treated mathematical concepts, in this regard, as analogous to empirical ones. For his use of the context principle to justify assuming that the domain of the individual variables comprises cardinal numbers, or value-ranges, required no more than that the relevant concept, of a cardinal number or of a value-range, have

determinate criteria of application and of identity; he saw no need for any *prior* circumscription of the domain. Precisely that is what we now take for granted as required. We know well enough what is needed for something to be recognised as a set or as an ordinal number, and when an entity given in a certain way is the same set or ordinal number as one given in another: but we certainly do not think of that as allowing us to form statements quantifying over all sets or all ordinal numbers and to treat them as having determinate truth-values. In the mathematical realm, reality cannot be left to blow all haziness away: we have to remove it ourselves by contriving adequate means of laying down just what elements the domain is to comprise.

This does not apply only to concepts like *set* and *ordinal number* for which contradiction results from treating their extensions as forming determinate domains of quantification, but to all means of specifying such a domain: the requirement of a prior specification of the domain, when interpreting a theory, formalised or unformalised, is general. The criterion of application of the concept *real number*, for example, might be said to be that whatever has a determinate relation of magnitude to any given rational is a real number, and the criterion of identity that, if the real number x is greater than or less than a rational number p if and only if the real number y is, respectively, greater than or less than p , then $x = y$. This is quite adequate to explain what is required of a specified mathematical entity for us to recognise it as a real number; but it does not suffice as a means of circumscribing a domain of quantification, when such quantification is to yield statements with determinate truth-values. It does not do so, because it fails to determine the limits of acceptable specification of something to be acknowledged as a real number: we still need a means of saying which real numbers the domain comprises.

The principal consequence of the set-theoretic paradoxes was that even platonists were compelled to allow that there are mathematical concepts whose extensions form hazy totalities: the concept of an ordinal number, for example. In this regard, Cantor saw much more clearly than Frege: but even he was in error in regarding the distinction between consistent and inconsistent totalities as an absolute one. So to regard it is to provoke intolerable perplexity. Consider what happens when someone is first introduced to the conception of transfinite cardinal numbers. A certain resistance has first to be overcome: to someone who has long been used to finite cardinals, and only to them, it seems obvious that there can only be finite cardinals. A cardinal number, for him, is arrived at by counting; and the very definition of an infinite totality is that it is impossible to count it. This is not a stupid prejudice. The scholastics favoured an argument to show that the human race could not always have existed, on the ground that, if it had, there would be no number that would be the number of all the human beings there had ever been, whereas for every concept there must be a number which is that of the objects falling under it. All the same, the prejudice is one that can be overcome: the beginner can be persuaded that

it makes sense, after all, to speak of the number of natural numbers. Once his initial prejudice has been overcome, the next stage is to convince the beginner that there are distinct cardinal numbers: not all infinite totalities have as many members as each other. When he has become accustomed to this idea, he is extremely likely to ask, 'How many transfinite cardinals are there?'. How should he be answered? He is very likely to be answered by being told, 'You must not ask that question'. But why should he not? If it was, after all, all right to ask, 'How many numbers are there?', in the sense in which 'number' meant 'finite cardinal', how can it be wrong to ask the same question when 'number' means 'finite or transfinite cardinal'? A mere prohibition leaves the matter a mystery. It gives no help to say that there are some totalities so large that no number can be assigned to them. We can gain some grasp on the idea of a totality too big to be counted, even at the stage when we think that, if it cannot be counted, it does not have a number; but, once we have accepted that totalities too big to be counted may yet have numbers, the idea of one too big even to have a number conveys nothing at all. And merely to say, 'If you persist in talking about the number of all cardinal numbers, you will run into contradiction', is to wield the big stick, not to offer an explanation.

What the paradoxes revealed was not the existence of concepts with inconsistent extensions, but of what may be called indefinitely extensible concepts. The concept of an ordinal number is a prototypical example. The Burali-Forti paradox ensures that no definite totality comprises everything intuitively recognisable as an ordinal number, where a definite totality is one quantification over which always yields a statement determinately true or false. For a totality to be definite in this sense, we must have a clear grasp of what it comprises: but, if we have a clear grasp of any totality of ordinals, we thereby have a conception of what is intuitively an ordinal number greater than any member of that totality. Any definite totality of ordinals must therefore be so circumscribed as to forswear comprehensiveness, renouncing any claim to cover all that we might intuitively recognise as being an ordinal. It does not follow that quantification over the intuitive totality of all ordinals is unintelligible. A universally quantified statement that would be true in *any* definite totality of ordinals must be admitted as true of all ordinals whatever, and there is a plethora of such statements, beginning with 'Every ordinal has a successor'. Equally, any statement asserting the existence of an ordinal can be understood, without prior circumscription of the domain of quantification, as vindicated by the specification of an instance, no matter how large. Yet to suppose all quantified statements of this kind to have a determinate truth-value would lead directly to contradiction by the route indicated by Burali-Forti.⁴

Better than describing the intuitive concept of ordinal number as having a

⁴ Abandoning classical logic is not, indeed, sufficient by itself to preserve us from contradiction if we maintain the same assumptions as before; but, when we do not conceive ourselves to be quantifying over a fully determinate totality, we shall have no motive to do so.

hazy extension is to describe it as having an increasing sequence of extensions: what is hazy is the length of the sequence, which vanishes in the indiscernible distance. The intuitive concept of ordinal number, like those of cardinal number and of set, is an indefinitely extensible one.⁵ Certain objects must be recognised outright as falling under such a concept: but what distinguishes it from all definite concepts is the principle of extendibility governing it. Russell's concept of a class not containing itself as a member is a prototypical example of an indefinitely extensible concept: for, once we form a definite conception of a totality W of such classes, it is evident that W cannot, on pain of contradiction, be a member of itself, and thus the totality consisting of all the members of W , together with W itself, is a more extensive totality than W of classes that are not members of themselves.

The principle of extendibility constitutive of an indefinitely extensible concept is independent of how lax or rigorous the requirement for having a definite conception of a totality is taken to be, although that will of course affect which concepts are acknowledged to be indefinitely extensible. It is clear that Frege's error did not lie in considering the notion of the extension of a concept to be a logical one, for that it plainly is. Nor did it lie in his supposing every definite concept to have an extension, since it must be allowed that every concept defined over a definite totality determines a definite sub totality. We may say that his mistake lay in supposing there to be a totality containing the extension of every concept defined over it; more generally, it lay in his not having the glimmering of a suspicion of the existence of indefinitely extensible concepts.

One reason why the philosophy of mathematics appears at present to be becalmed is that we do not know how to accomplish the task at which Frege so lamentably failed, namely to characterise the domains of the fundamental mathematical theories so as to convey what everyone, without preconceptions, will acknowledge as a definite conception of the totality in question: those who believe themselves already to have a firm grasp of such a totality are satisfied with the available characterisations, while those who are sceptical of claims to have such a grasp reject them as question-begging or unacceptably vague. An impasse is thus reached, and the choice degenerates into one between an act of faith and an avowal of disbelief, or even between expressions of divergent tastes. Moreover, the impasse seems intrinsically impossible of resolution; for fundamental mathematical theories, such as the theory of natural numbers or the theory of real numbers, are precisely those from which we initially derive

⁵ The idea of an indefinitely extensible concept was expressed by Russell at the end of section I of 'On some Difficulties in the Theory of Transfinite Numbers and Order Types' (reprinted from *Proceedings of the London Mathematical Society*, series 2, vol. 4, 1906, pp. 29–53 in B. Russell, *Essays in Analysis*, ed. D. Lackey, London, 1973, pp. 135–64) as follows:

the contradictions result from the fact that . . . there are what we may call *self-reproductive* processes and classes. That is, there are some properties such that, given any class of terms all having such a property, we can always define a new term also having the property in question.

our conceptions of different infinite cardinalities, and hence no characterisation of their domains could in principle escape the accusation of circularity.

Now what is it for a totality to be infinite? More exactly, what is it for it to be *intrinsically* infinite, that is, for the very conception of that totality to entail its infinity? It is for us always to have a means of finding another element of the totality, however many we have already identified; the new element will be characterised in terms of those previously identified. For a non-denumerable totality like the real numbers, Cantor's diagonal construction provides just such a means, given any denumerable set of elements.⁶ A denumerable totality, likewise, is one for which we can find a further element, given any initial segment of it: the similarity between Frege's proof of the infinity of the sequence of natural numbers and the foregoing demonstration that the concept *class not a member of itself* is indefinitely extensible can hardly escape notice. We have a strong conviction that we *do* have a clear grasp of the totality of natural numbers; but what we actually grasp with such clarity is the principle of extension by which, given any natural number, we can immediately cite one greater than it by 1. A concept whose extension is intrinsically infinite is thus a particular case of an indefinitely extensible one. Assuming its extension to constitute a definite totality – one of which we can form a sharp conception and which forms a determinate domain of quantification – may not lead to inconsistency; but it necessarily leads to our supposing that we have provided definite truth-conditions, independently of whether or not we can prove them, for statements that cannot legitimately be so interpreted. The hypothesis that the domains of the fundamental mathematical theories are given by what are in fact indefinitely extensible concepts explains why we are at such a loss to supply uncontentious characterisations of their domains.

It springs to the lips to retort that the argument begs the question: it depends, in the one case, on identifying totalities of which we can form a definite conception with denumerable ones, and, in the other case, on identifying them with finite ones. It is, however, this reply that begs the question. These totalities are those from which we *derive*, respectively, our conception of one of the cardinality of the continuum and our conception of an infinite one: until we have a conception of the real numbers (or of the set of all sets of natural numbers), we have only a conception of denumerable totalities to go on; and, until we have the conception of the totality of all natural numbers,

⁶ 'The same applies to his original, though less well-known, proof of the non-denumerability of the continuum: see Michael Hallett, *Cantorian Set Theory and Limitation of Size*, Oxford, 1984, pp. 74–6. Given an enumeration $a_1, a_2, a_3, \dots$ of real numbers in an interval $[a, b]$, with $a_1 = a$ and $a_2 = b$, we can form sequences $a^{(1)}, a^{(2)}, \dots$ and $b^{(1)}, b^{(2)}, \dots$, where $a^{(i+1)}$ is the first element in the enumeration in the interval $(a^{(i)}, b^{(i)})$ and $b^{(i+1)}$ is the first element in the interval $(a^{(i+1)}, b^{(i)})$. If these sequences terminate, their last terms determine an interval within which no element of the enumeration can lie; if not, they either determine such an interval or a number (their common limit) that cannot occur in the enumeration. As Hallett notes, this proof is similar in principle to Cantor's first proof of the non-denumerability of the second number class (of the denumerable ordinals).

we have only a conception of finite totalities to go on. Admittedly, the lame characterisation of the totality which supplies our usual ground for supposing that we do have a definite conception of it does not always appeal to the notion of completing the process of extension. The standard characterisation of the totality of natural numbers, as consisting of everything attainable from 0 by reiterating the successor operation, does have this form; but the characterisation of the real numbers as comprising those corresponding to all cuts in the rational line does not. The question is, however, whether there is any way of achieving a more precise characterisation of the highly unsurveyable totality of *all* such cuts; it is only by appeal to a principle of extension that we convince ourselves that this cannot be done by any method of enumerating them.

The requirements for characterising an indefinitely extensible concept are far less exigent than those for giving a description of a definite totality as one of which we have a clear grasp. A criterion of application (and a criterion of identity) are indeed required: it must be stated what, in general, is demanded of something for us to recognise it as falling under the concept. As already noted, this asks for much less than a precise circumscription of a totality; if we choose to explain the concept *real number* in a Dedekindian manner (probably not the best choice) by saying that a real number is required to have determinate relations of magnitude to rationals, we say nothing about the manner in which an object having such relations is to be specified, but simply leave any purported specification to be judged on its merits when it is offered. The concept requires a base of objects satisfying the criterion of application and unquestionably well specified, and a principle of extendibility. The former is easily provided; the latter will be stated in terms of a definite totality of objects falling under the concept, where it is again left to be judged, in any proposed case, whether we have such a definite totality or not. When the concepts of *natural number* and of *real number* are regarded as indefinitely extensible ones, our grasp of them is beyond question; it is only when they masquerade as definite concepts that any attempt to characterise them becomes vague or circular.

This diagnosis breaks the impasse; but, of course, at a price. Quantification over the objects falling under an indefinitely extensible concept obviously does not yield statements with determinate truth-conditions, but only ones embodying a claim to be able to cite an instance or an effective operation; and the logic governing such statements is not classical, but intuitionistic. Adoption of such a solution therefore entails a revision of mathematical practice in accordance with constructivist principles. Such a revision would have been abhorrent to Frege: it is unclear that it would be less of a betrayal of the fundamental principles of his philosophy of mathematics than his own eventual expedient of reducing arithmetic to geometry.

Frege's contribution to the philosophy of mathematics

Frege's attempt to justify the existence of mathematical objects was not simply a failure that left us where we were before: it left us with a precise range of options. We cannot simply ignore the problem, but must choose between them. If we set aside intuition either of our mental creations or of the abstract realm, there are only three. We can maintain that we do have intuitive conceptions of the real numbers, of Cantor's second number-class, and perhaps even of a model for Zermelo-Fraenkel set theory, sufficiently determinate to confer senses on the propositions of the relevant theories which will warrant applying to them the principle of bivalence. This heroic stance will validate an invocation of the context principle just as Frege intended; but it is far from compelling. It is futile simply to claim to *have* an intuition; it must be capable of being conveyed to others by being expressed in language or symbolism. No one denies that attempts to convey such intuitive models succeed in expressing something; but the claim that they convey a conception of a domain of quantification sufficiently definite to warrant attributing to statements involving quantification determinate truth-values is, to most, quite unconvincing. Alternatively, we can side with the constructivists in admitting mathematical objects without claiming to be able to circumscribe precisely in advance which such objects are to be recognised; propositions concerning them must then be construed as obeying intuitionistic, not classical, logic. And, finally, we can join with the nominalists in thinking that mathematics can dispense with objects altogether. The attempt actually to dispense with them within mathematics would involve a more far-reaching transformation of the subject as currently practised than a constructivist revolution. If a demonstration that dispensing with them would be in principle possible whenever mathematics was applied within an extra-mathematical theory were capable of being given only piecemeal, theory by theory, mathematics would lose its generality and its autonomy. If, for each mathematical theory, such a demonstration could be given in advance for all physical theories satisfying certain general conditions, the question would arise on what grounds this was preferable to the second, constructivist, option. Investigation might reveal that a constructivist version of a given mathematical theory was perfectly adequate for the applications made of it within natural science. If so, then, for anyone who agrees with Frege that it is applicability alone that raises mathematics from the rank of a game to that of a science, a constructivist reformulation of the mathematical theory would clearly be preferable to an indirect justification in terms of the property of conservativeness. If, on the other hand, it proved that the classical version of the mathematical theory had a substantial effect upon the scientific theory, the question would not yet be settled: for, on the hypothesis that everything derivable by aid of the classical theory could in principle be derived from the scientific theory alone, the classical force of the mathematical theory

would have already to be embodied in that scientific theory. The question would then arise whether a version of it divested of that classical force (and thus of realistic metaphysical assumptions) would not be scientifically preferable. These questions have scarcely been raised, let alone answered, by either mathematicians, philosophers or physicists.

These speculations have taken us very far from Frege's work. His failure to make any enquiry into the validity of classical logic, as applied to mathematical theories, is the one big lacuna – as opposed to the big error – in his philosophy of mathematics. It is one for which he can hardly be blamed. He can probably be reproached for his increasing inability to see through the errors and confusions in others' expositions of their ideas to the merit of those ideas themselves; considering the disappointments that disfigured his entire life, we can only regret, not blame. He left behind him a philosophy of arithmetic which he himself believed, for the last two decades of his life, to have been a total failure, the only valuable part of his work, in his eyes, having been in formal and philosophical logic. That philosophy of arithmetic was, indeed, fatally flawed; but it had an incontestable clarity, so that, even where it was mistaken, it pointed very precisely to where the problems lay. But it did much more than that. Frege's polemic against formalism contained a definitive refutation of that deadening philosophical interpretation of mathematics. To important questions in the philosophy of mathematics, above all those concerning the application of mathematics, the fruitfulness of deductive reasoning and the nature of mathematical necessity, his work provided, if not full-dress answers, at least sketches of what must be the correct answers; later philosophers have come nowhere near his partial success in answering those questions, and have frequently failed even to address them. Above all, Frege provided the most plausible general answer yet proposed to the fundamental question, 'What is mathematics?', even if his answer cannot yet be unarguably vindicated. For all his mistakes and omissions, he was the greatest philosopher of mathematics yet to have written.

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